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When Euler taught logic to the Princess by drawing the overlapping circles and circles within circles, he was also illustrating the well-known fact that human beings have to learn the principles of formal logic. They do not, it is generally agreed, always or even often think logically. So, if behavioral scientists are to study and understand the processes of ordinary, everyday thinking, they must develop new approaches, including the recognition that thinking is colored by emotional attitudes. By using a system based on a definition of cognitive elements and the relations between them, a psychological mathematical model has been constructed for use in the rigorous experimental analysis of attitudinal cognition.

SYMBOLIC PSYCHO-LOGIC: A MODEL OF ATTITUDINAL COGNITION

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SEVERAL lines of theoretical and research interest have gradually been converging upon the study of "cognitive structure," especially in the area of social attitudes. Attitudes, by the definitions of Smith, Bruner and White (18), Peak (15), Rosenberg (17), Green (7) and many others, involve both affective and cognitive components. These components interact intimately with one another, so that cognitions about attitudinal objects are not felt to be meaningfully analyzable without consideration of affective forces. Theorists are reluctant even to consider cognitive units of an attitude apart from other cognitive units, preferring to treat cognition as "structured" into meaningful wholes (13, 19). Krech and Crutchfield (12, p. 152) define attitude globally as "an enduring organization of motivational, emotional, perceptual and cognitive processes with respect to some aspect of the individual's world." They state (p. 108) "Each of our perceptions does not lead 'a life of its own' but is embedded in an organization of other perceptions—the whole making up a specific cognitive structure." Smith, Bruner, and White (18, p. 286), in discussing attitude measurement, caution, "Recent advances in the theory and technique of attitude measurement . . . have yet to take this complexity [of opinions] into account. Characteristically, they have conceived of attitudes as a matter of pro-ness and con-ness. . . . We would question whether it is reasonable to expect them to generate the dimensions required for an adequate description of opinion. [An adequate description] comes rather from intensive explorations. . . ." Asch (2) is another author who has employed a holistic concept of attitude structure. He seeks to explain certain attitude change phenomena in terms of the "restructuring" of the attitudinal object (3). Rokeach (16) treats structure as an individual difference variable of cognitive style, defining one extreme, dogmatism, as "a relatively closed cognitive organization of beliefs and disbeliefs about reality . . . which . . . provides a framework for patterns of intolerance and qualified tolerance toward others."

This Gestalt emphasis, motivated by the desire to construct an accurate model of the phenomenal organization of attitudes, has underscored the difficulties inherent in attempts to deal analytically with a set of cognitive units or elements. Nevertheless, if psychologists are to talk about "cognitive structure," they must do something about it; there must be a correspondence between theory and some sort of research operation. At least two important recent theoretical developments, Festinger's (6) "dissonance theory," and Heider's (10) "theory of cognitive balance," deal conceptually with the

effects of organizing forces and affective forces upon cognitive elements. Both theories are concerned with changes in cognitive structure. Festinger's theory deals mainly with inconsistencies between belief and action, and attempts to specify certain circumstances under which there will be more or less change in belief as an outgrowth of cognitive "dissonance," due to such inconsistencies. The research operations which are used to test Festinger's theory are not addressed to the details of cognitive structure, but rather to single-response predictions derivative from the theory. Heider's theory is addressed to structural details of cognition, but there has been no research operation available to investigate these details with particular attitude contents. On the other hand, the recently proposed methodological devices of Rosenberg (17) and Abelson (1) are not completely anchored in a general theory of attitudes. A fresh approach to the measurement of cognitive structure is clearly called for.

This paper proposes one such approach. We have drawn some inspiration from Cartwright and Harary's (5) objective system for the examination of "structural balance," from Heider's (10) ideas on cognitive balance, and are also intellectually indebted to Zajonc (20), who has suggested clever procedural devices in eliciting cognitive material. Otherwise, the system to be proposed is our own, albeit with a strong historical tether. The paper is divided into two parts: the "Psychological Model" and the "Mathematical System."

THE PSYCHOLOGICAL MODEL

Cognitive elements

Human thought, for all its complexity and nuance, must involve some cognitive representation of "things," concrete and abstract. These things or concepts are the *elements* of our system. Though it does not seem absolutely necessary, we shall for convenience assume that individuals can attach some sort of verbal labels to the elements of their thinking. We will refer, then, to cognitive elements by verbal labels.

We may distinguish three broad classes of elements: Actors, Means (Actions, Instrumentalities), and Ends (Outcomes, Values). These classes are not completely exhaustive nor mutually exclusive. We propose this classification merely to suggest the variety of possible cognitive elements. To exemplify each class, consider the illustrative issue: "Having an honor system at Yale." The following elements were among those frequently elicited from Yale students in a pilot study.

Actors: myself, the Faculty, the Administration, the Student Body, a certain minority of students, the honest student. . . .

Means: the honor system, other honor systems, the present examination system, cheating, reporting those who cheat, social pressure from other students. . . .

Ends: the feeling of being trusted, mature moral standards, loyalty to friends, the University's reputation, having well-run examinations. . . .

Note that all elements are expressed here substantively to reflect their "thing-like" character.

Cognitive relations

Of all the conceivable relations between cognitive elements, we choose to consider only four: *positive*, *negative*, *null*, and *ambivalent*. These will be denoted *p*, *n*, *o*, and *a*. (See also "Mathematical System"). We again conveniently assume that cognitive materials of a relational sort can be verbally labeled. Some typical examples of relations between elements follow:

		positive (*p*)	*negative* (*n*)	*null* (*o*)
Actor vs.	Actor	likes; supports	dislikes; fights	is indifferent to
	Means	uses; advocates	opposes; undermines	is not responsible for
	End	possesses; aims for	inhibits; aims against	is indifferent to
Means vs.	Actor	helps; promotes	hinders; insults	does not affect
	Means	is equivalent to	is alternative to; counteracts	is unrelated to
	End	brings about	prevents	does not lead to
End vs.	Actor	serves; is vital to	is inimical to	does not interest
	Means	justifies	obviates	cannot ensue from
	End	is consistent with	is incompatible with	is unconnected to

Ambivalent relations (*a*) are defined as conjunctions of positive and negative relations; they are psychologically secondary or "derived," and have been omitted from the above list.

Note that relations are expressed here as verbs.

Certain omissions from the above scheme may occur to the reader. Relations like "is next in line to," "is north of," etc. are certainly not included. Other phrases which imply relationship without specifying its sign, e.g., "depends upon," "is connected with," etc., are difficult to classify. Furthermore, notions of time sense, moral imperative, and conditionality or probability, e.g., "might have acted upon," "probably will help," "ought to seek," etc. are not dealt with explicitly. Time sense and conditionality are beyond the scope of the present treatment. We deal here only with cognitions of the psychological present, and temporarily set aside the past, conditional past, future, and conditional future. As for the other exceptions to the scheme, note that their phrasing tends to be affectless. Dispassionate descriptions of, say, the mating habits of flamingoes or the operation of a nuclear reactor or the economic situation in Viet Nam would probably contain many relations impossible to classify. But such descriptions are reportorial, not attitudinal. When cognition is invested with affect, when the Actors, Means, and Ends are responded to emotionally, then the relations become classifiable in terms of the present system. Our intent is to be able to code all relations occurring in attitudinal cognitions into these four broad categories.

Cognitive units or sentences

Cognitive units are built out of pairs of elements, connected by a relation. That is, the basic "sentences" of attitudinal cognition are of the form

$$A r B$$

where A and B are elements and *r* is a relation. Many sentences which at first seem more complicated than the simple A*r*B unit may be reduced to such a unit by broadening the definition of an element. For example, consider the sentence,

"Nasser (A) insists on (*p*) all Suez tolls (B) belonging to (*p*) Egypt (C)." (Here *p* denotes a positive relation.) This sentence, symbolically, is A*p*(B*p*C). But regard (B*p*C) as a new element D, the broader conception "all Suez tolls belonging to Egypt." Then we have simply A*p*D.

In this way, we reduce our catalogue of basic sentences to four:

A *p* B
A *n* B
A *o* B
A *a* B

The conceptual arena. The structure matrix

For the convenience of both theory and operation, we conceive of an attitude as being defined over a certain delimited though perhaps large *conceptual arena.* We restrict attention, in other words, to those elements which are (phenomenally) relevant to the given issue or attitude object.

Operationally, what one can do is this. Following Zajonc (20), we ask the subject to list all the words or short phrases that come to mind as he mulls over a given topic, such as "Having an Honor System at Yale" or "The State Department Ban on Reporters Going to Red China." Subjects will typically write several phrases in rapid-fire order, then pause, give one or two more, and finally stop. These words or phrases are not always "noun-like," but by simple instructions to the subject, they can be reworded satisfactorily and be treated subsequently as the cognitive elements. This procedure yields the conceptual arena idiosyncratic to each subject.[1]

After the arena is defined, the relations between pairs of elements may be mapped. We conceptualize a matrix setting forth the relations between each element and every other. This matrix we denote the *structure matrix, R* (see the "Mathematical System"). As an illustration of a structure matrix,

[1] For some experimental purposes, it is more convenient to define a common conceptual arena for a group of subjects. Some topics yield low overlap between subjects, but other topics possess sufficient overlap so that it is worthwhile to construct a group arena encompassing the most generally salient elements.

consider the following:

STRUCTURE MATRIX; TOPIC: HAVING AN HONOR SYSTEM AT YALE

	Ego	Honest student	Report cheater	Feel trust	Cheat. by few	Honor syst.
Ego (the subject himself)	*p*	*p*	*n*	*p*	*n*	*o*
The honest student	*p*	*p*	*n*	*p*	*o*	*p*
Reporting cheaters	*n*	*n*	*p*	*o*	*n*	*p*
Feeling trusted	*p*	*p*	*o*	*p*	*o*	*p*
Cheating by a few	*n*	*o*	*n*	*o*	*p*	*n*
An honor system	*o*	*p*	*p*	*p*	*n*	*p*

The entries in this matrix were elicited by instructing a subject to fill in the *middles* of sentences linking his elements (the six listed). He wrote these "open-middled" sentences:

The honest student *feels very reluctant about* reporting cheaters.
The honest student *possesses* the feeling of being trusted.
The honor system *is fine for* the honest student.
The honor system *promotes* the feeling of being trusted.
Cheating by a few *is cut down by* reporting cheaters.
The honor system *is harmed by* cheating by a few.
The honor system *involves* reporting cheaters.

The verb forms in these sentences were coded[2] into the relations *p* and *n* shown in the matrix. In addition, the subject himself was asked whether he felt favorable, neutral, or unfavorable toward each element. His responses are shown in the first row (column) of the matrix.

All relations between elements and themselves are taken to be positive. Also, relations are considered to be symmetric; i.e., if A*r*B, then B*r*A. Our preliminary work does not suggest a pressing need to revise these assumptions.

The structure matrix is our basic description of attitudinal cognition. One may wish to superimpose further refinements upon the structure such as the intensity of different relations, the confidence with which they are held, time perspective, and so on. In this paper, we answer only the simplest question, "In what way can the structure matrix be used to characterize the fundamental structural varieties of attitudinal cognition, apart from specific content?"

[2] In order to circumvent the coding problem, one may require the subject to choose his verb forms from a precoded list. This version is presently being attempted. Full details of the entire procedure will be set forth elsewhere.

Thinking. Psycho-Logic

Individual cognitive units may originate in various ways—through exposure to written information, through social pressure, emotional need, thinking, etc. Once originated, these units may be manipulated in thought.

We propose a set of rules by which an individual imputes or discovers new symbolic sentences by combining two old sentences with an element in common. We hypothesize that *these rules apply only when the individual thinks about the topic*, or "rehearses the arguments" (11, p. 129).

Rule 1. A*p*B and B*p*C implies A*p*C.
Rule 2. A*p*B and B*n*C implies A*n*C.
Rule 3. A*n*B and B*n*C implies A*p*C.
Rule 4. A*o*B and B*r*C implies nothing about the relation between A and C, irrespective of *r*.
Rule 5. If A*p*C and A*n*C are both implied, or if one is held initially and the other implied, then A*a*C. This is the definition of the ambivalent relation.
Rule 6. A*a*C and C*p*D implies A*a*D.
Rule 7. A*a*C and C*n*D implies A*a*D.
Rule 8. A*a*C and C*a*D implies A*a*D.

Each rule above has several equivalent forms not listed, arising from the symmetry assumption A*r*B = B*r*A.

Though we have given formal status to these rules,[3] note that the verbal corollaries are not necessarily logical at all. Exemplify Rule 3 by the sentences:

India (A) opposes (*n*) U. S. Far Eastern policy (B)

[3] The entire set of rules may be organized into one with the aid of Tables 1 and 2 of The "Mathematical System." Letting *R* denote the structure matrix, derived or imputed sentences may be found from the matrix multiplication of *R* by itself. The powers R^2, R^3, etc. yield imputations which are successively more removed from the original sentences.

U. S. Far Eastern policy (B) is directed against (*n*) Communism (C).
Therefore, India (A) is in favor of (*p*) Communism (*C*).

Such "reasoning" would mortify a logician, yet it can be found in much this form inside of millions of heads. Thus we speak of the formal system as *psycho-logic* rather than as *logic.*

We assume that thinking about a topic *must be motivated before it will occur.* Some possible motivating conditions[4] would be these:

1. Pressure to reach a decision on the topic.
2. Socially derived needs to appear informed on the topic, to converse well about it, to win over others, etc. Anticipation of the relevant social situations would motivate thinking.
3. Relevance of the topic to needs, conflicts, and persisting preoccupations. Activation of such processes would generate pressure to think.
4. A general "cognitive style" of the individual such that thinking per se is satisfying. For such individuals, mere mention of the topic might motivate thinking.

Suppose that a given individual is motivated to think about a given topic. He already possesses certain relevant cognitive units. Using psycho-logic, he imputes new units. These new units may or may not be compatible with his existing units. Referring back to the India-China example, if the individual had originally thought, "India (A) resists the influence of (*n*) Communism (C)," then he would be confronted with the coexistence of A*p*C and A*n*C. By Rule 5, this is expressed A*a*C, the ambivalent relation, which might translate back into words thus:

The relationship between India (A) and Red China (C) is very confusing (*a*).

On the other hand, had he originally held A*p*C, then the imputed sentence A*p*C would reinforce rather than contradict his original view.

[4] The potency of these various motivating conditions could be subjected to empirical test, using before and after measures of cognitive structure and experimental manipulation of motivating conditions in-between. One such experiment has in fact been carried out by Zajonc (20), using a different theoretical context. See his discussion of "cognitive tuning."

Balance and imbalance

Certain structure matrices are characterized by the interesting property that no amount of thinking (i.e., imputing of new relations according to the rules above) leads the individual into any inconsistency (i.e., ambivalent relation). The cognitive structures represented by such matrices are said to be *balanced.* When the structure matrix is such that one or more ambivalent relations will be discovered in thinking, the structure is said to be *imbalanced.* (Our statement of the mathematical condition for cognitive balance is given in the "Mathematical System.") In common-sense terms, a balanced cognitive structure represents a "black and white" attitude. The individual views some elements as good and the other elements as bad. All relations among "good elements" are positive (or null), all relations among "bad elements" are positive (or null), and all relations between good and bad elements are negative (or null).

Redressing imbalance

Heider (10), Festinger (6), Osgood and Tannenbaum (14), and others have postulated that individuals strive to reduce or redress cognitive imbalance, or "dissonance." This is probably especially true for attitudinal cognitions. It is important to note, however, that potential imbalance will remain undiscovered by an individual unless he is motivated to think about the topic and in fact does so. Assuming these necessary preconditions, suppose that an individual does come upon a cognitive inconsistency in his attitude. What can he do about it? Three things:

1. Change one or more of the relations.
2. Redefine, "differentiate," or "isolate" one or more elements.
3. Stop thinking.

A simple example will illustrate the first two methods. The third method is self-explanatory. Take the three elements, E, C, G: Ego (a Yale student), Having Co-eds at Yale, and Getting Good Grades. The subject might tell us,

"I'm for having co-eds at Yale." (E*p*C)
"I want good grades." (E*p*G)
"Having co-eds at Yale would undoubtedly interfere with getting good grades." (C*n*G)
This attitudinal cognition

$$R = \begin{array}{c|ccc} & \text{E} & \text{C} & \text{G} \\ \hline \text{E} & p & p & p \\ \text{C} & p & p & n \\ \text{G} & p & n & p \end{array}$$

is imbalanced. Never having been forced to take a consistent stand on the issue, however, our subject may readily tolerate (or even be unaware of) this imbalance. Now suppose that the issue is hotly debated and our subject thinks. The imbalance becomes apparent and he seeks a balance-producing resolution.

Method 1. He alters any one of the three relations, by abandoning the desire for good grades, by opposing the admission of co-eds, or by rationalizing to the effect that "Co-eds do not really interfere with getting good grades (in fact, they enhance the chances, etc.)"

Method 2. (The following is one of various possibilities.) He differentiates the concept "Getting good grades" into "Getting A's" and "Getting C's" and then reasons that while co-eds may interfere with getting A's, they don't interfere with getting C's, and what he really wants is not to get A's but to get C's.

It is assumed that under sufficient pressure to continue thinking, the individual will try Methods 1 and 2, presumably seeking a relatively effortless means to achieve balance. If these attempts fail, because certain relations are resistant to change and certain elements are difficult to redefine, the individual may resort to Method 3 which is to stop thinking. If, however, strong pressures, internal or external, do not permit him to stop thinking, he will re-examine the topic, seeking a more complex utilization of either or both of the first two methods. And so on. With extremely strong pressure to continue thinking, some cognitive units will in all probability ultimately yield to one attack or another.[5]

With weak pressure and a structure that is highly resistant to change, the individual will most likely stop thinking, and his attitudinal structure will revert to its state before he started thinking.

In other words, we envision an extensive hierarchy of cognitive solutions to the problem of reducing imbalance. Experimental prediction of outcome is extremely difficult under these circumstances, and represents a considerable challenge. The next section presents a crude preliminary step toward meeting the challenge.

Experimental prediction of cognitive changes

Our present machinery is not refined enough to permit predictions with respect to Method 2 above, "redefining the elements." It would appear feasible, however, to restrict experimentally the availability of this alternative (the arguments of Asch [3] to the contrary) and focus upon Method 1.

The question we pose for ourselves is this: "Given an imbalanced structure[6] with highly stably defined elements, can we predict which particular set of relations in the whole conceptual arena will be changed in order to achieve balance?"

The mathematical system presented later in detail is helpful in providing what we hope will be an approximate answer. In that system, the *complexity of imbalance* of a structure is defined as the *minimum number of changes of relations necessary to achieve balance.*[7] The system also implies a

[5] So-called "conversion experiences" may be representative of the extreme case: very extensive cognitive changes are characteristic of this phenomenon.

[6] One may provoke imbalance experimentally by strong persuasive communication aimed at a given cognitive unit. We are presently launching an experiment of this type.

[7] Cartwright and Harary (5) have proposed a different index for degree of structural imbalance. The present index is simpler to compute, and has also the appealing feature that it refers to the dynamic property of change in structure. Harary, in a personal communication, has indicated that he has recently and independently hit upon an index equivalent to the present one. He calls it the "line index of balance." A "line" in his terminology is a relation in our terminology. His "line index of balance" is the minimal number of lines (relations) whose negation yields balance. He

way of identifying which particular changes constitute the minimum set, or, when there are alternative minimal sets, identifies all of them. In addition, the "next to minimal" sets of changes can be identified, and so on.

Working under the highly tentative heuristic assumption that all relations are equally resistant to change, the least effortful balancing operation on an imbalanced structure is the one that requires the fewest changed relations. A predicted set of changes can easily be generated under this assumption.

The steps involved in the analysis are:

1. Write down the structure matrix.

2. Find the row (column) with the largest plurality of *n*'s over *p*'s, exclusive of the *p* in the main diagonal of the matrix. Change all *n*'s to *p*'s and *p*'s to *n*'s, in both the row and the corresponding column (leaving the diagonal entry *p* unchanged). Ignore the *o* entries.

3. Repeat the previous step on a new row (column).

4. Continue the change operations on selected rows and columns until the number of *n*'s in the matrix cannot be further reduced.[8]

This irreducible minimum number defines the complexity of imbalance. Duplication due to symmetry is avoided by counting *n*'s on one side of the diagonal only. The entries where the *n*'s appear identify the minimal set. If the minimum number can be attained in more than one way, the sets so identified are alternative "solutions." If the minimum number is zero, then the structure is balanced. The entire procedure is based upon Theorem 13 of the "Mathematical System."

The method will now be illustrated with the Yale student's structure on "Having an honor system at Yale," presented in an earlier section.

His structure matrix was:

	1. Ego	2. Honest student	3. Report cheater	4. Feel trust	5. Cheat by few	6. Honor syst.
1. Ego	*p*	*p*	*n*	*p*	*n*	*o*
2. The honest student	*p*	*p*	*n*	*p*	*o*	*p*
3. Reporting cheaters	*n*	*n*	*p*	*o*	*n*	*p*
4. Feeling trusted	*p*	*p*	*o*	*p*	*o*	*p*
5. Cheating by a few	*n*	*o*	*n*	*o*	*p*	*n*
6. An honor system	*o*	*p*	*p*	*p*	*n*	*p*

Row 5 contains three *n*'s and one *p*. We then carry out the change operation on Row 5 and Column 5, carrying *p* into *n* (except in the diagonal) and *n* into *p*. The result is:

	1.	2.	3.	4.	5.	6.
1.	*p*	*p*	*n*	*p*	*p*	*o*
2.	*p*	*p*	*n*	*p*	*o*	*p*
3.	*n*	*n*	*p*	*o*	*p*	*p*
4.	*p*	*p*	*o*	*p*	*o*	*p*
5.	*p*	*o*	*p*	*o*	*p*	*p*
6.	*o*	*p*	*p*	*p*	*p*	*p*

Both remaining *n*'s lie in Row 3. Three *p*'s also lie in this row, but one of them is the unchanging diagonal element, so that essentially it is two *n*'s versus two *p*'s. The change operation on Row 3 and Column 3 would yield

	1.	2.	3.	4.	5.	6.
1.	*p*	*p*	*p*	*p*	*p*	*o*
2.	*p*	*p*	*p*	*p*	*o*	*p*
3.	*p*	*p*	*p*	*o*	*n*	*n*
4.	*p*	*p*	*o*	*p*	*o*	*p*
5.	*p*	*o*	*n*	*o*	*p*	*p*
6.	*o*	*p*	*n*	*p*	*p*	*p*

The number of *n*'s here is also two. It is evident that the number of *n*'s cannot be further reduced by the change operation. It is also evident upon further inspection that no other set of change operations will yield as few as two *n*'s. Thus the structure matrix is of complexity *two* and there are

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points out that negation in the sense of changed sign and negation in the sense of deletion or elimination are equivalent insofar as balancing is concerned. Where we say "change of relation," this may be taken to mean change from *p* to either *n* or *o*, and change from *n* to either *p* or *o*.

[8] A certain amount of trial-and-error may be required before reaching a final decision. This decision is not automatic; the change operation is sometimes useful on a row and column with a plurality of *p*'s over *n*'s—a temporary *increase* in the number of *n*'s can lead to an ultimate further reduction in the minimum number of *n*'s.

two alternate minimal solutions to the balancing problem.

The first solution tells us that had the subject's initial relations between elements 3 and 1, and 3 and 2 not been as they were, there would not have been any imbalance. In other words, achieving balance requires changing the relations between elements 3 and 1, and elements 3 and 2. Looking back at his original structure matrix, this means that were he to *favor* reporting cheaters, or at least not be opposed, and also regard honest students as favoring or not opposing reporting cheaters, his structure would be balanced. The "good elements" would be *Ego, The honest student, Reporting cheaters, Feeling trusted*, and *The honor system*, while *Cheating by a few* would be a "bad element." In short, "We honest students would all feel trusted in an honor system. A few cheaters might violate the system. but they should be reported."

The second solution tells us that the subject must change the relations between elements 3 and 5, and elements 3 and 6 to achieve balance. In other words, were he to view *Reporting cheaters* as not identified with the honor system, and as in any case ineffective in cutting down *Cheating by a few* his structure would be balanced. *Ego, The honest student, Feeling trusted*, and *The honor system* would be "good elements," and *Reporting cheaters* and *Cheating by a few*, "bad elements." A hypothetical quote would read, "We honest students would all feel trusted in an honor system. A few cheaters might violate the system, but I wouldn't feel right about reporting them and it probably wouldn't work anyway. There are a few lousy guys who will always try to foul things up."

We make no claim that this subject would necessarily make one or the other of these changes in striving for a balanced structure. We only claim that if the strengths of all relations are equal, the above changes would be preferred for their simplicity.

We are now working on various means for empirical measurement of the strength of relations. Incorporation of the strength variable in the model will represent an important refinement.

THE MATHEMATICAL SYSTEM

The mathematical system is designed specifically for dealing analytically with the structure of attitudinal cognitions. There is some exercise of invention in suiting definitions and devices to the problem at hand; otherwise, the system is a self-contained axiomatic treatment of a standard mathematical nature. Many of the results parallel those of Harary (8, 9) although his mathematical treatment differs from the present one.

Consider a set E of four "relations": p, n, o, and a (the positive, negative, null, and ambivalent relations of the Psychological Model) with the following properties under the fundamental operations of addition and multiplication:

$$\left.\begin{array}{ll}[1] & r + o = r \\ [2] & r + r = r \\ [3] & r + a = a\end{array}\right\} \text{ for all } r \in \mathrm{E}$$

$$[4] \quad p + n = a$$

$$\left.\begin{array}{ll}[5] & r \cdot p = r \\ [6] & r \cdot o = o\end{array}\right\} \text{ for all } r \in \mathrm{E}$$

$$[7] \quad r \cdot a = a \quad \text{for } r = p, n, \text{ or } a$$

$$[8] \quad n \cdot n = p$$

Each operation is taken to be commutative, so that

$$\left.\begin{array}{ll}[9] & r_1 + r_2 = r_2 + r_1 \\ [10] & r_1 \cdot r_2 = r_2 \cdot r_1\end{array}\right\} \text{ for all } r_1, r_2 \in \mathrm{E}$$

These fundamental definitions may readily be scanned in the following addition and multiplication tables:

TABLE 1

ADDITION TABLE FOR RELATIONS

+	p	n	o	a
p	p	a	p	a
n	a	n	n	a
o	p	n	o	a
a	a	a	a	a

TABLE 2

MULTIPLICATION TABLE FOR RELATIONS

·	p	n	o	a
p	p	n	o	a
n	n	p	o	a
o	o	o	o	o
a	a	a	o	a

It will be noted that the inverse operations, subtraction and division, are not uniquely defined for all pairs of elements. For example, the equation

$$p + x = p$$

cannot be uniquely solved for x. A similar situation holds for the equations

$$a \cdot x = a$$

and

$$o \cdot x = o.$$

However, the associative and distributive properties of multiplication and addition hold in this system.

$$\left.\begin{array}{ll} [11] & (r_1 + r_2) + r_3 \\ & = r_1 + (r_2 + r_3) \\ [12] & (r_1 \cdot r_2) \cdot r_3 \\ & = r_1 \cdot (r_2 \cdot r_3) \\ [13] & r_1 \cdot (r_2 + r_3) \\ & = r_1 \cdot r_2 + r_1 \cdot r_3 \end{array}\right\} \text{ for all } r_1, r_2, r_3 \in E$$

Now consider an $N \times N$ matrix R with elements $r_{ij} (i = 1, 2, \cdots N;\ j = 1, 2 \cdots N)$ such that:

[R1] all $r_{ij} = p$, n, or o
[R2] $r_{ii} = p$ for all i
[R3] $r_{ij} = r_{ji}$ for all i and j.

These stipulations define a *structure matrix*. Denote the class of all $N \times N$ structure matrices R by $\mathfrak{R}^{(N)}$, or when the context clearly refers to any arbitrary fixed N, simply $\mathfrak{R}$.

Define the pseudo-determinant[9] $\dagger R \dagger$ of a matrix R as

$$[14] \qquad \dagger R \dagger = \sum_{\text{all } \phi} \prod_{i=1}^{N} r_{i\phi_i}$$

where ϕ is a permutation on the integers $i = 1, 2, \cdots N$, and ϕ_i is the integer into which i is carried by the permutation. The rules of addition and multiplication are as given previously. Otherwise, this idiosyncratic definition corresponds to the usual definition of a determinant (4) except that no algebraic signs are attached to the individual products involved in the summation. They are all positive, so to speak. The prefix "pseudo-" is intended to warn of this distinction from an ordinary determinant.

[9] Often referred to as the "permanent."

The pseudo-determinant provides the basis for an interesting partition of the class $\mathfrak{R}$ of all structure matrices. Note that specification [R2] for R-matrices requires all elements on the main diagonal to be p. Thus the term in $\dagger R \dagger$ corresponding to all $\phi_i = i$ must be the product $(p \cdot p \cdots p) = p$, regardless of the nature of the off-diagonal elements of a given R. Given that at least one term in the summation in [14] equals p, it readily follows from Table 1 and condition [11] that

$$[15] \qquad \dagger R \dagger = p \text{ or } a, \qquad \text{for all } R \subset \mathfrak{R}$$

The former is true if and only if *no* term in the summation [14] equals n.

Definition. If $\dagger R \dagger = p$, then R is said to be *balanced*. If $\dagger R \dagger = a$, then R is said to be *imbalanced*.

The definition in these terms is completely equivalent to the definition of balance in the psychological model and to the definition of "structural balance" given by Cartwright and Harary (5). Proof of this equivalence is straightforward but space-consuming and will be omitted here. The heart of the matter is the equivalence between permutations in the pseudo-determinant and "cycles" of cognitive elements.

Definition.

$\mathfrak{R}_0$ is the class of all balanced R-matrices.
$\mathfrak{R}_a$ is the class of all imbalanced R-matrices.
Clearly the union $\mathfrak{R}_0 \cup \mathfrak{R}_a = \mathfrak{R}$.

A further partition of the class $\mathfrak{R}_a$ is possible, giving rise to an index of the complexity or degree of imbalance in any given imbalanced R.

To accomplish this, it is convenient to define a class $\mathfrak{T}$ of square *transformation matrices* T with elements t_{ij} such that:

[T1] All t_{ij} = the element s ("sameness") or the element c ("change")
[T2] $t_{ii} = s$ for all i.
[T3] $t_{ij} = t_{ji}$ for all i and j.

The specifications [T1–3] are parallel to the specifications [R1–3], and indeed, the elements s and c will be defined in such a way as to parallel the elements p and n.

Multiplication is the main operation in which the elements s and c enter, as follows:

[16] $s \cdot s = s$
[17] $s \cdot c = c \cdot s = c$
[18] $c \cdot c = s$
[19] $s \cdot r = r$, for $r = p, n,$ or o.
[20] $c \cdot p = n$
[21] $c \cdot n = p$
[22] $c \cdot o = o$

The elements s and c may readily be understood as representing "sameness" and "change" respectively. A transformation matrix T embodies a set of changes which may be applied to a structure matrix R, in a way to be described later.

The operation of addition is also defined on c and s. It is commutative and associative.

[23] $c + c = c$
[24] $s + s = s$
[25] $c + s = a$
[26] $a + c = a$
[27] $a + s = a$

The addition of c or s to p, n, or o is not defined.

The pseudo-determinant of T is defined as it was for R in equation [14].

[28] $$\dagger T \dagger = \sum_{\text{all } \phi} \prod_{i=1}^{N} t_{i\phi_i}$$

There are only two possibilities for the value of $\dagger T \dagger$, just as there were for $\dagger R \dagger$ by equation [15].

[29] $\dagger T \dagger = s$ or a, for all $T \subset \mathfrak{T}$

The former is true if and only if *all* terms in the summation [28] equal s.

Definition. If $\dagger T \dagger = s$, then T is said to be *passive*. If $\dagger T \dagger = a$, then T is said to be *active*.

Denote by $\mathfrak{T}_0$ the class of all passive T-matrices and by $\mathfrak{T}_a$ the class of all active T-matrices. Clearly the union

$$\mathfrak{T}_0 \cup \mathfrak{T}_a = \mathfrak{T}$$

Theorem 1 (The "Interchange Condition"). A matrix T is passive if and only if, for all h, i, j, k,

$$t_{hj}t_{ik} = t_{hk}t_{ij}\,.$$

Proof To prove sufficiency, we use the definition [28]. Consider the term

$$\prod_{i=1}^{N} t_{i\phi_i}$$

in the summation, corresponding to some permutation ϕ. If no interchange of a pair of column subscripts alters the value of a product, then it must follow that no permutation of N column subscripts alters the product. In particular, the permutation ϕ^{-1} would not. But ϕ^{-1} carries $\prod_{i=1}^{N} t_{i\phi_i}$ into $\prod_{i=1}^{N} t_{ii}$, which equals s by [T2]. Therefore $\prod_{i=1}^{N} t_{i\phi_i}$ necessarily equals s. This being true for all terms in the summation, thus $\dagger T \dagger = s$, and T is passive.

To prove that any violation of the "interchange condition" $t_{hj}t_{ik} = t_{hk}t_{ij}$ necessarily implies T nonpassive, suppose that at least one set h, i, j, k were found with $t_{hj}t_{ik} = c \cdot t_{hk}t_{ij}$. (Every case of violation of the interchange condition must be expressable in this way, since elements of T can only equal s or c). Now consider any term Π_1 of [28] in which $t_{hj}t_{ik}$ appears. Let the product of the remaining elements in the term be denoted Π_0, that is, $\Pi_1 = t_{hj}t_{ik}\Pi_0$. By definition of the pseudo-determinant, the product Π_0 also appears in another term, namely $\Pi_2 = t_{hk}t_{ij}\Pi_0$. Whatever the value of Π_0, it follows that $\Pi_1 = c \cdot \Pi_2$, whence $\dagger T \dagger = a$, since not all terms in [28] equal s.

Theorem 2 The interchange condition of Theorem 1 is satisfied if and only if, for each pair h and i of rows in T, either

[i] $t_{hj} = c \cdot t_{ij}$ for all j, or
[ii] $t_{hj} = s \cdot t_{ij}$ for all j.

Rows satisfying [i] we shall call *antagonistic*, and satisfying [ii], *compatible*.

Proof. Any columns j and k of two antagonistic rows, h and i, would by [i] display

$$t_{hj} = ct_{ij}$$
$$t_{hk} = ct_{ik}$$

Whence

$$t_{hj}(ct_{ik}) = t_{hk}(ct_{ij}),$$

or

$$c(t_{hj}t_{ik}) = c(t_{hk}t_{ij}).$$

Thus

$$t_{hj}t_{ik} = t_{hk}t_{ij}\,,$$

the interchange condition. A similar deriva-

tion holds for compatible rows, with s appearing wherever c does above.

For the proof of necessity, note that a pair of rows which is neither antagonistic nor compatible will violate the interchange condition, since then some j and k will exist for which

$$t_{hj} = ct_{ij}$$
$$t_{hk} = st_{ik}$$

and $$t_{hj}t_{ik} = ct_{hk}t_{ij}\,.$$

Corollary 1. A T-matrix is passive if and only if every pair of rows is either antagonistic or compatible.

Corollary 2. A T-matrix is passive if and only if its rows can be partitioned into two subsets, α and $\bar{\alpha}$, such that all pairs of rows in α are compatible, all pairs in $\bar{\alpha}$ are compatible, and all pairs with one member in each subset are antagonistic.

Next we proceed to define a special operation of one T-matrix on another.

Definition. The *application* $TU = W$, where $T, U \subset \mathfrak{T}$ are matrices of the same order (N), is defined simply by

$$w_{ij} = t_{ij}u_{ij} \quad \text{for all } i, j.$$

The definition immediately implies $W \subset \mathfrak{T}$, since the conditions [T1–3] must be satisfied by the elements w_{ij} of W. In other words,

Theorem 3. The class $\mathfrak{T}^{(N)}$ is closed under the operation of application.

The following theorems may be proved with dispatch.

Theorem 4. Application is commutative.

$$TU = UT, \text{ with } T, U \subset \mathfrak{T}^{(N)}$$

Theorem 5. Application is associative.

$$(TU)V = T(UV), \text{ with } T, U, V \subset \mathfrak{T}^{(N)}$$

Definition. The *identity transformation* $S \subset \mathfrak{T}$ is given by the matrix with all $t_{ij} = s$. (Note that $S \subset \mathfrak{T}_0$; that is, the identity transformation is a passive transformation). Clearly $ST = T$ for all $T \subset \mathfrak{T}$, by virtue of [16] and [17].

Theorem 6. Every $T \subset \mathfrak{T}$ is self-inverse, i.e.,

$$T \cdot T = S$$

This follows immediately from the definition of application and rules [16] and [18].

Theorem 7. The class $\mathfrak{T}_0^{(N)}$ is closed under application.

Proof. If $T \subset \mathfrak{T}_0^{(N)}$, then by Theorem 1

$$t_{hj}t_{ik} = t_{hk}t_{ij} \quad \text{for all } h, i, j, k.$$

If $U \subset \mathfrak{T}_0^{(N)}$, then

$$u_{hj}u_{ik} = u_{hk}u_{ij} \quad \text{for all } h, i, j, k.$$

Then for $W = TU$, we will have for all h, i, j, k

$$\begin{aligned} w_{hj}w_{ik} &= (t_{hj}u_{hj})(t_{ik}u_{ik}) \\ &= (t_{hj}t_{ik})(u_{hj}u_{ik}) \\ &= (t_{hk}t_{ij})(u_{hk}u_{ij}) \\ &= (t_{hk}u_{hk})(t_{ij}u_{ij}) \\ &= w_{hk}w_{ij}\,, \end{aligned}$$

and thus $W \subset \mathfrak{T}_0^{(N)}$.

Definition. Let ${}_iT^*$, the "allegiance transform on i," denote the T-matrix with

(i) $t_{ij} = t_{ji} = c$ for a given i and all $j \neq i$.

(ii) all other elements $= s$.

By Corollary 1 of Theorem 2, ${}_iT^* \subset \mathfrak{T}_0$, since the above definition implies that row i is antagonistic to all other rows, while any two rows $g \neq i$ and $h \neq i$ are compatible. From this and Theorem 7, we infer

Theorem 8. An application of the form

$$U = \prod_{i \in \alpha} {}_iT^*,$$

where α is any subset of the integers 1 to N, implies $U \subset \mathfrak{T}_0$.

Corollary 1. All $U \subset \mathfrak{T}_0$ formed as above must satisfy Corollary 2 of Theorem 2. The subset α above and the subset α of the previous Corollary are in fact one and the same. This may be shown by a straightforward albeit clumsy tracing through of the elements u_{hk} of U for the separate cases $(h,\ k \in \alpha)$, $(h,\ k \in \bar{\alpha})$, $(h \in \alpha,\ k \in \bar{\alpha})$, $(h \in \bar{\alpha},\ k \in \alpha)$.

Now, since specification of various partitions α generates an exhaustive catalogue of passive T-matrices by Corollary 2 of Theorem 2, and since a passive T-matrix with any given partition α may in fact be constructed by successive application on

matrices of the form T^*, we have the important

Theorem 9. All passive T-matrices can be constructed by applications of the form

$$\prod_{i \in \alpha} {}_iT^*$$

Rewording this, we may say that the *set of allegiance transforms* ${}_iT^*$ *is a "basis" for the class* $\mathfrak{T}_0$.

Returning to the analysis of R-matrices, we establish the

Definition. The application $TR = Q$, where $T \subset \mathfrak{T}^{(N)}$, $R \subset \mathfrak{R}^{(N)}$ is given by

$$q_{ij} = t_{ij} \cdot r_{ij} \quad \text{for all } i, j$$

This directly implies $Q \subset \mathfrak{R}^{(N)}$, by the rules [$R$1–3] and [19–22].

The application of a transformation matrix thus carries one structure matrix into another. This is an important tool, since it enables the analysis of R-matrices to proceed in terms of analysis of their transforms, which is often simpler.

Theorem 10. $(TU)R = T(UR)$, with T, $U \subset \mathfrak{T}^{(N)}$ and $R \subset \mathfrak{R}^{(N)}$

Proof follows immediately from the above definition.

Let us examine what happens when a passive T is applied to a balanced R.

Theorem 11. A transformation $T \subset \mathfrak{T}_0$ applied to an $R \subset \mathfrak{R}_0$ yields some $Q \subset \mathfrak{R}_0$. That is, *passive transforms of balanced R-matrices are balanced.* (Recall from the psychological model that balanced R-matrices represent "black and white" attitudes. A passive transform will change one or more elements from "black" to "white," or "white" to "black," but leave the state of balance undisturbed.)

Proof. The pseudo-determinant of Q is

$$\begin{aligned} Q &= \sum_{\text{all } \phi} \prod_{i=1}^{N} q_{i\phi_i} \\ &= \sum_{\text{all } \phi} \prod_{i=1}^{N} (t_{i\phi_i} \cdot r_{i\phi_i}) \\ &= \sum_{\text{all } \phi} \left\{ \prod_{i=1}^{N} t_{i\phi_i} \right\} \left\{ \prod_{i=1}^{N} r_{i\phi_i} \right\}, \end{aligned}$$

since all t and r elements commute. But the former product is s for all ϕ when $T \subset \mathfrak{T}_0$, and the latter product is p for some ϕ and possibly o for others, though it is never n when $R \subset \mathfrak{R}_0$ (see the remarks following conditions [29] and [15]).

Therefore,

$$\dagger Q \dagger = p, \qquad \text{and } Q \subset \mathfrak{R}_0 .$$

Next, consider the case of imbalanced R-matrices. Given some $R \subset \mathfrak{R}_a$, it is always possible to find at least one $X \subset \mathfrak{T}$ such that $XR = Q \subset \mathfrak{R}_0$. In particular, one could choose $X \subset \mathfrak{T}$ with the property that $x_{ij} = c$ wherever $r_{ij} = n$, and s otherwise. Then all entries in Q would be either p or o and clearly $\dagger Q \dagger = p$, ergo $Q \subset \mathfrak{R}_0$. Now consider applications of the form $(WX)R$, with $W \subset \mathfrak{T}_0$ (and thus $WX = V \subset \mathfrak{T}$, by Theorem 3).

$$\begin{aligned} (WX)R = W(XR) &= WQ \\ &= \text{some } Q' \subset \mathfrak{R}_0 , \end{aligned}$$

using Theorems 10 and 11. The construction WX enables the formation of many transforms carrying a given imbalanced R into a balanced one, once the special transform X is established. The various transforms $V = WX$ differ from each other in the number of c-entries they contain, suggesting a

Definition. The *complexity* of an imbalanced R-matrix is the integer m, determined as follows:

[i] Construct $X \subset \mathfrak{T}$ with $x_{ij} = c$ wherever $r_{ij} = n$ and s otherwise.

[ii] Form all possible applications $V = WX$, with $W \subset \mathfrak{T}_0$

[iii] Count the number of c-entries in each V, dividing by two to avoid duplication due to symmetry.

[iv] Denote by m the *minimum* count thus determined.

The complexity of imbalance, then, is the *minimum number of changes necessary to produce balance.*

Denote the class of all $R \subset \mathfrak{R}_a$ with complexity m by $\mathfrak{R}_m$. This provides the partition, promised earlier, of the class $\mathfrak{R}_a$ into $\mathfrak{R}_1$, $\mathfrak{R}_2$, $\mathfrak{R}_3$, etc.

Theorem 12. A transformation $T \subset \mathfrak{T}_0$ applied to an $R \subset \mathfrak{R}_m$ yields some $P \subset \mathfrak{R}_m$.

That is, *complexity of imbalance is invariant under a passive transformation.*

Proof. The complexity of R, denoted say m_1, is defined on the class of matrices WX, where $W \subset \mathfrak{T}_0$ and X is such that $XR = Q \subset \mathfrak{R}_0$.

Now consider the complexity m_2 of the transform $P = TR$. We establish an analogue Y of the previous X by choosing $Y = XT$. Then,

$$YP = (XT)(TR) = XSR = XR = Q \subset \mathfrak{R}_0.$$

Thus m_2 is defined on the class of matrices WY, with $W \subset \mathfrak{T}_0$. But $WY = WXT = (WT)X$.

Now we have m_1 defined on WX, and m_2 defined on $(WT)X$. But since the only requirement on W is that it be a member of the class $\mathfrak{T}_0$, and since by Theorem 7 the application (WT) must be a member of $\mathfrak{T}_0$ inasmuch as $T \subset \mathfrak{T}_0$, it follows that the class of (WT) is identical to the class of W. (They both correspond to $\mathfrak{T}_0$.) Therefore the class of all WX is identical to the class of all $(WT)X$, and $m_1 = m_2$.

Theorem 13. Complexity of imbalance is invariant under all "allegiance" transformations of the form

$$\prod_{i \in \alpha} {}_iT^*.$$

This follows immediately from Theorems 9 and 12.

The operational simplifying procedure recommended in the psychological model for determining complexity of imbalance is a consequence of this theorem. "Changes of allegiance" do not alter the complexity of imbalance, so that one may reverse p's and n's in any successive set of rows and columns until the complexity of imbalance is apparent.

Many other theorems may be derived from this system. However, limitations of space make it imperative to conclude here.

We state one final result, without proof.

Theorem 14. The maximum complexity possible for an $N \times N$ R-matrix is

$$m_{\max.}(N) = \frac{N^2 - 2N + \delta}{4} \qquad \delta = \begin{cases} 1 \text{ if } n \text{ odd} \\ 0 \text{ if } n \text{ even} \end{cases}$$

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(Manuscript received May 25, 1957.)

Before a social aggregate can be examined for its status as an organic system its status as entity must be evaluated. Indices of common-fate, similarity, and proximity may be appropriate to this task. Social groups as entities do not have an epistemological status different from such middle-sized entities as stones and rats, but are apt to be fuzzier, less discrete, less multiply confirmed, and in this sense less real. The degree of entitativity and the possibility of a sociology at a level of analysis separate from psychology is a matter for empirical determination rather than a priori decision.

COMMON FATE, SIMILARITY, AND OTHER INDICES OF THE STATUS OF AGGREGATES OF PERSONS AS SOCIAL ENTITIES[1]

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THERE is currently considerable interest in efforts to apply concepts about "systems" at the level of social groupings. But for such applications to be meaningful, we should be able to test the hypothesis in specific instances that an aggregate of persons behaves as a system. We should be able to find instances in which the hypothesis of system is confirmed, and instances in which it is not, or in which the systemicity is of lesser degree. Too frequently concepts of "system" or "homeostatis" or "dynamic structure" are made axiomatic and lose their status as testable hypotheses. Too frequently such concepts are used in a way which provides an infinite flexibility in *ad hoc* explanations, and thus lose that essential rigidity which might make them testable. Yet the hypothesis of system is clearly testable in some instances. A pound of hamburger is clearly less of a system than a one-pound living white rat, and there are many measurements possible which would document this difference. If biologists have seldom provided lists of such criteria, it is not because the difference is undemonstrable, but rather because it is so obviously demonstrable as to be a waste of laboratory effort to verify.

Those interested in treating social and political organizations as systems are most frequently offered the biological organism as an example of a system. This is true both of recent conceptualizations in the behavioral science movement and of those available at the founding of modern social science. For those interested in a functional theory of social organization, Herbert Spencer's *Principles of Sociology, Part II, The Inductions of Sociology* (20), can still be recommended. The behavioral scientist will find it unacceptable in many minor details, but also profoundly modern in spirit and brilliantly suggestive of still untested research leads. Spencer is also free from the unwarranted ethical conclusion which has alienated so many from the organismic analogy, to wit, that not only are societies organism-like, but that it is good for them to become more so. As a militant anti-nationalist and pacifist, resisting the temptation to identify with the glories of Queen Victoria's expanding empire, he used the analogy as a source of concepts about process and function, rather than as a pseudo-scientific prop for an ethic. To quote from his closing chapter:

> Here let it once more be distinctly asserted that there exist no analogies between the body

[1] The completion of this paper was made possible by the author's participation in the Behavioral Science Conference held at the University of New Mexico, Summer, 1957, under Project AF 49(638)-33 sponsored by the Behavioral Science Division, Air Force Office of Scientific Research, ARDC, Professor Paul A. F. Walter, principal investigator.

politic and a living body, save those necessitated by that mutual dependence of parts which they display in common. Though, in foregoing chapters, sundry comparisons of social structures and functions to structures and functions in the human body, have been made, they have been made only because structures and functions in the human body furnish familiar illustrations of structures and functions in general. The social organism, discrete instead of concrete, asymmetrical instead of symmetrical, sensitive in all its units instead of having a single sensitive centre, is not comparable to any particular type of individual organism, animal or vegetal. All kinds of creatures are alike in so far as each exhibits cooperation among its components for the benefit of the whole; and this trait, common to them, is a trait common also to societies. Further, among individual organisms, the degree of co-operation measures the degree of evolution; and this general truth, too, holds among social organisms. Once more, to effect increasing co-operation, creatures of every order show us increasingly complex appliances for transfer and mutual influence; and to this general characteristic, societies of every order furnish a corresponding characteristic. These, then are the analogies alleged: community in the fundamental principles of organization is the only community asserted (20).

Before we can go ahead with testing the hypothesis that political units or social organizations are organism-like systems, there is a prior question which must be answered. Spencer states the problem in his first chapter entitled, "What is a Society." This chapter is but two pages long. To set the stage more specifically it can be quoted in full:

> This question has to be asked and answered at the outset. Until we have decided whether or not to regard a society as an entity; and until we have decided whether, if regarded as an entity, a society is to be classed as absolutely unlike all other entities or as like some others; our conception of the subject-matter before us remains vague.
>
> It may be said that a society is but a collective name for a number of individuals. Carrying the controversy between nominalism and realism into another sphere, a nominalist might affirm that just as there exist only the members of a species, while the species considered apart from them has no existence; so the units of a society alone exist, while the existence of the society is but verbal. Instancing a lecturer's audience as an aggregate which by disappearing at the close of the lecture, proves itself to be not a thing but only a certain arrangement of persons, he might argue that the like holds of the citizens forming a nation.
>
> But without disputing the other steps of his argument, the last step may be denied. The arrangement, temporary in the one case, is permanent in the other; and it is the permanence of the relations among component parts which constitutes the individuality of a whole as distinguished from the individualities of its parts. A mass broken into fragments ceases to be a thing; while, conversely, the stones, bricks, and wood, previously separate, become the thing called a house if connected in fixed ways.
>
> Thus we consistently regard a society as an entity, because, though formed of discrete units, a certain concreteness in the aggregate of them is implied by the general persistence of the arrangements among them throughout the area occupied. And it is this trait which yields our idea of a society. For, withholding the name from an ever-changing cluster such as primitive men form, we apply it only where some constancy in the distribution of parts has resulted from settled life.
>
> But now, regarding a society as a thing, what kind of thing must we call it? It seems totally unlike every object with which our senses acquaint us. Any likeness it may possibly have to other objects, cannot be manifest to perception, but can be discerned only by reason. If the constant relations among its parts make it an entity; the question arises whether these constant relations among its parts are akin to the constant relations among the parts of other entities. Between a society and anything else, the only conceivable resemblance must be one due to *parallelism of principle in the arrangement of components.*
>
> There are two great classes of aggregates with which the social aggregate may be compared—the inorganic and the organic. Are the attributes of a society in any way like those of a not-living body? or are they in any way like those of a living body? or are they entirely unlike those of both?
>
> The first of these questions needs only to be asked to be answered in the negative. A whole of which the parts are alive, cannot, in its general characters, be like lifeless wholes. The second question, not to be thus promptly answered, is to be answered in the affirmative. The reasons for asserting that the permanent relations among the parts of a society are analogous to the permanent relations among the parts of a living body, we have now to consider (20, pp. 447–448).

Forgiving him his haste and superficiality, the non sequitur in the second sentence of the last paragraph, and his mistaken notion about the social life of primitive man, I believe Spencer to be right in his ordering of the problem:

1. Among actual or potential aggregates of persons, there are certain aggregates which meet criteria of being "entities," and other aggregates which do not. The distinction is capable of empirical representation.

2. Among those actual or potential aggregates which meet the criteria of being

entities there are some which meet the further and more specific criteria of being organic systems. These criteria are likewise capable of operational specifications.

But I believe his treatment of the first problem is inadequate, and that its specification poses a major obstacle to the development of usable systems theory in the social sciences. For the anthropologist in isolated areas, the specification of the social units of tribe and clan may pose little difficulty. The status of entity and system, the useful boundaries of aggregation, may be as apparent to him as to a vertebrate biologist. But for groups in the modern western world, the problems become difficult. If we are to test whether "it" is a system, we need to know what the "it" we are dealing with is, what its boundaries are, where it begins and leaves off. It may be, for example, that a modern metropolitan area is in certain features a homeostatic system—but how can we test the hypothesis unless we have criteria for defining a metropolitan area? Political units such as wards, cities, townships, and counties, might fail to meet the test merely because they represented arbitrary fragments of social entities.

The perception of entities

It is to this first problem of Spencer's that I wish to address myself. I wish to do so by reverting to my role as psychologist and raising the more general problem as to how we "perceive" entities. The paper might well be retitled: "Perceiving invisible entities such as social groups." In this elaboration, I am to some extent following another great sociologist, like Herbert Spencer in receiving less attention today than he might, although otherwise he may seem a strange bedfellow. May I quote from Stuart Rice's *Quantitative Methods in Politics* (19), in his chapter most appropriately labeled "a statistical view of a perceptual world."

> It is true that some of the data reported by human sense organs seem more substantial than others. One sees a saxophone and hears the atmospheric disturbances to which it gives rise, but one cannot see or hear or touch or smell the group relationships which in some perplexing manner bind the members of the jazz orchestra together. The first surmise that each member is wholly independent of the others in thought and action eventually breaks down, and it is perceived that they have organization, but the organization seems less tangible than the saxophone.
>
> In actuality the two types of data are not as dissimilar as they seem. The existence of the saxophone is only inferred from sense impressions of it. If the concert patron should become mentally ill and have an hallucination in which the saxophone played a part, the testimony of his senses would later be rejected, if he were restored to sanity, because of the absence of corroboration for the hallucinatory evidence. Corroboration would be required in the form of other sense impressions received directly by the patient, emanating from or relating to the saxophone, or indirectly from other individuals who in turn received sense impressions relating to the instrument in question. When all possible corroboration is secured, belief in the existence of any material thing remains an inference. Belief in the existence of a group relationship is of the same character. It is an inference, based upon a variety of sense impressions concerning the behavior of individuals who are involved.
>
> All data from the perceptual world turn out to be evidences concerning reality, but not reality itself; or, if preferred, such evidences are reality and the latter is nothing more. Whatever the preferred form of statement, it remains true that what is known of a hypothetical world without one's own individual mind is universally subject to error and inexactitude. Scientific method conceptualizes the perceptual data and treats them *as if* they were real and exact entities. This methodological process, like a great deal more of scientific method, is essentially fictional. Its justification is to be found in the results to which it leads. Hence it is as valid for the scientist to speak of a social group as to speak of an ounce of ether, provided he can do something further with the idea. All he knows of either is what he infers through the mediumship of his own or (by a secondary process) of another person's sense impressions (19, pp. 23–24).

In his subsequent chapters on "Group cohesion and likeness" and "The identification of blocs in small political bodies" he puts into operational terms the question, "when is an aggregate of persons an entity?" While I shall make little use of his specific solutions, the perspective on the problem is the same.

One other source must be acknowledged: Karl Deutsch's chapter on "Mapping and measurement" in his *Political Community at the International Level* (9) represents the sophisticated statement of a behavioral scientist who uses the concepts of system, boundary, and organization in such a way

that empirically verifiable exemplifications of his concepts are made explicit. One quotation from this chapter will help further set the stage for the discussions.

> The concept of reality will be employed here in its operational meaning. It will be appropriate to speak of reality in those cases where such limited tests, qualitative and quantitative, as are at our disposal indicate some historical or social structure from which we can predict the outcome of other tests, including kinds of tests not yet devised. Tentative descriptions of historical or social processes or structures will of course be in terms of symbols, such as maps or words or numbers, which will describe the inferred structures to which they apply. The more verifiable such descriptions prove to be (that is, the wider the range of mutually independent testing operations by which they are confirmed), the more reality may be ascribed to the tentative models or constructions. In other words, models will be more realistic the larger the number of mutually independent operations, present or future, by which they are confirmed (9, p. 47).

You will note a common preoccupation with the "reality" of social units in these three quotations spanning the 80 years since 1876. These represent, however, only mild and methodologically sophisticated statements of the "realist" position. They perhaps should better be called the "as real as" position, recognizing as they do the fallible and mediate nature of our knowledge of common-sense objects as well as of social entities. As such they could also be the expression of a consistent nominalism. More strident denials (1) and affirmations (23) are of course available. The promising aspect of these three statements is that they all propose empirical tests of the "reality" of social units, and it is this feature that I want to elaborate. But there lies behind these chronic protests and denials a stubborn psychological problem which will continue to plague us. The natural knowledge processes with which we are biologically endowed somehow make objects like stones and teacups much more "real" than social groups or neutrinos, so that we are offended by the use of the same term "real" to cover both instances, and are thus tempted to an attitude of conventionalism toward the latter which we would reject if applied to the former. Two sources of the feeling can be suggested. First: according to certain objective criteria to be discussed below, groups are in most instances less "solid," less multiply confirmed, of less sharp boundaries, less "hard." Second, and most important, we have evolved in an environment in which the identification of certain middle-sized entities was both useful and anatomically possible (14). As a product of this evolutionary process we have the marvelously effective mechanism of vision, which, within a limited range of entities, analyzes entitativity[2] so rapidly and vividly that all other inferential processes seem in contrast indirect, ponderous, and undependable. More important, the visual process is so powerful and seemingly direct that we usually do not stop to notice the inferential steps involved in it and the nature of the clues employed. My proposal is that we look to the empirical clues of entity used in the visual perception of middle-sized physical entities and then employ these clues in the analysis of social aggregates as entities.

Wertheimer, (24, 26 p. 625) has given us a basic list of principles of perceptual organization, demonstrated through research on vision under limiting conditions of low entitativity. His inquiry was as to the factors which lead discrete elements to be perceived as parts of a whole organization, and which determine the organization they will be perceived as parts of. These are his general principles insofar as characteristics of the stimulus elements are concerned:

1. Proximity: elements close together are more likely to be perceived as parts of the same organization.

2. Similarity: similar elements are more likely to be perceived as parts of the same organization.

3. Common fate: elements that move together in the same direction, and otherwise in successive temporal observations

[2] Entitativity: the degree of being entitative. The degree of having the nature of an entity, of having real existence. (The present writer regrets adding two suffix syllables to the word *entitative*, already three fourths suffixes. Probably the "ity" of entity at one time connoted "degree of" being, but that term was so long ago preempted for categorical uses allowing no consideration of degree that some new term must be employed.)

share a "common fate" are more likely to be perceived as parts of the same organization.

4. Pregnance, good continuation or good figure: elements forming a part of a spatial organization or pattern, as a line or more complex form, tend to be perceived as a part of the same unit.

These represent by and large objective or operationally specifiable aspects of the stimulus field. In the analysis which follows there will be an effort to state the diagnostic principles which they imply in such fashion as to bypass the direct visual perception of wholes, and in such fashion as to be equally applicable to stones and social groups.

Pregnance or good continuation may be expanded into the notion of closed figure, or completed boundary. How else could the five blind men of the parable have convinced themselves that they dealt with but one entity, with but one and the same elephant, except by extending their surface exploration until they had described a closed surface, at which point they would have joined hands and achieved intersubjective communicability and agreement on the entity and its shape? I will attempt to so state the first three clues so that they generate boundaries which may then be examined for closure or completeness.

Common fate as a source of boundaries

To start with the stone. Let us at some one moment index the molecules (or some larger, more convenient unit, such as cubic centimeters) of substance in a volume including the stone and adjacent substances. Let us subsequently over a series of occasions record the locus of each unit of substance. From these records let us compute between each pairing of the units a "coefficient of common fate" which will be larger the more frequently the two units have been in the same general region at the same time. If our substance sample is in fact discontinuous, then the coefficients will be very high among those bits of substance which we may subsequently identify as an entity, relatively low between these bits "of it" and other bits which are parts of other "things." The rigidity, solidity, permanence which "real things" are reputed to have are expressions of this "common fate," which excepting for adamant is never absolute, is always subject to rupture.

Let us similarly tag a sample of persons, large enough so that all members of one group are included and many more. Let us observe the locations of these persons on a broad sample of occasions, and compute between each person and each other an index of common fate. Such indices could be similarly used to delineate entities. To be sure, the intra-entity coefficients would never run quite as high as those within the stone, and the inter-entity coefficients would run higher, for small groups sampled within a complex western society. The boundaries, in other words, would be empirically less distinct, less sharp. The entities of stone and group are equally "real" insofar as the formal epistemological procedures for knowing them are concerned, but the stone when known turns out to be more solid, harder, and of sharper edges than the group. Similarly, a band of gypsies is empirically harder, more solid, more sharply bound than the ladies aid society, and the high-school basketball team during basketball season falls somewhere in between, as judged by the average level of the intra-entity common fate coefficients and the ratio between these and the intra-to-extra-entity (inter-entity) common-fate coefficients. On grounds such as these, the zoologist of social insects has scarcely more trouble identifying colonies of bees, ants, and termites than he does in identifying a single bee as an entity, provided he is privileged to observe the behavior of the colony's members over a period of time. (But then, insect groups have sharper boundaries than do human groups.)

The use of coefficients of common fate, differing from zero to unity, enables us to deal with entitativity as a matter of degree, and even in the physical world intergrades occur. Thus between the teapot and the teapot lid the inter-entity coefficients are high, though not quite as high as the intra-entity coefficients. Lower, but still high, are the inter-entity coefficients between cup and saucer. The intra-entity common fate coefficients are higher for a platoon than for a

battalion, and higher for a destroyer crew than for either, etc.

If the three preceding paragraphs have succeeded in conveying the purpose of the common fate coefficient and the general mode of application intended, it is perhaps appropriate to spell out in more detail how such a coefficient might be constructed. It seems easiest to begin for both stones and social groups with common fate in a single physical dimension. Figure 1 shows the scatter diagram for the common fate coefficient for two particles over a population of 12 occasions (t_1–t_{12}). Computing a product-moment coefficient on this scatter diagram would give an index of approximately .99. It seems likely that the two particles are parts of the same entity. Note that had the two particles been labeled spots on opposite surfaces of a rolling stone, the scatter diagram would have been fuzzier, in that sometimes particle *A* would have been west of particle *B*, and sometimes east. But if the general range of movement was many magnitudes greater than the diameter of the stone, the coefficient would approach unity. Note that it is possible although perhaps unlikely for common fate coefficients to be unity for particles widely separate in space, and this fact has been suggested by the location of the regression line of Figure 1 off from the diagonal of identity values. Thus the geographical common fate coefficients between two Rockettes at opposite ends of the chorus line may be as high or higher than that between any two adjacent girls. Note that movement of both *A* and *B* in longitude must occur before the computation of a common fate coefficient is possible. However, the movement of *A* may be much less in absolute distance than that of *B*, i.e., the slope be other than 45°, and the coefficient still be unity.

This mode of approach becomes cumbersome, even at the conceptual level, because it generates a multiplicity of common fate coefficients, one for each measurement dimension, one for longitude, one for latitude, one for elevation, etc. The physicist may be able to suggest an elegant unification in terms of location in phase space, but short of that, the working psychometrician might

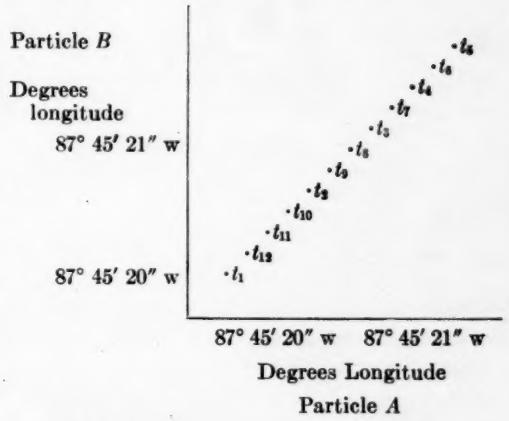

FIG. 1. HYPOTHETICAL COMMON FATE PLOT BETWEEN PARTICLE *A* AND PARTICLE *B*.

suggest a composite common fate coefficient. For this, each scatter diagram would be remetricized in standard score units separately for *A* and *B* values, and the scatter diagrams then superimposed: each measure at each occasion providing a separate entry, and a single coefficient being computed for the whole set.

The band of gypsies, the basketball team, and the ladies aid society would probably all be diagnosable in terms of a common fate coefficient based upon spatial parameters alone. Other groups might not be, including members of an Indian pueblo, for want of sufficient variability in space or for other reasons. The essence of the common fate coefficient is co-variability in time, and other variable parameters such as activity level, temperature, reflected light, morale, hedonic tone, nutritional status, etc. could be employed.

There was considerable implausibility in the initial supposition that we label all of the particles of the rock. In practice one might paint labels on the surface portions of the stone and compute common fate coefficients among these, and between these and the labeled surface sections of other stones, but the very magnitude of the common fate coefficients, the very solidity (and brittleness) of the stone would have prevented the intrusion of the labeling process for the interior segments, or this intrusion would have fragmented the stone. The

TABLE 1

COMMON FATE COEFFICIENTS FOR THE PARTICLES OF THREE ENTITIES

		1.			2.			3.			
		A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*	*K*
1.	*A*											
	B	.90										
	C	.91	.92									
	D	.93	.90	.91								
2.	*E*	.55	.52	.54	.53							
	F	.52	.55	53	.52	.85						
	G	.53	.54	.51	.55	.84	.86					
3.	*H*	.15	.10	.16	.17	.32	35	.33				
	I	.18	.11	.14	.16	.34	.31	.36	.95			
	J	.15	.13	.13	.16	.38	.35	.34	.96	.94		
	K	.17	.15	.11	.17	.36	.35	.38	.97	.95	.96	

hypothetical procedure would thus have been in actuality more applicable at the social group level than at the level of stones.

The absolute level of the coefficients is not sufficient to the diagnosis of groups, as that level will vary depending upon the breadth of the sampling of occasions and the amount of activity, the degree to which the gravel pit has been stirred. The boundaries will always be relative, and the degree of correlation which is interpreted as intra-entity will depend upon the general level of the coefficients, both intra- and inter-entity which obtain. The relativity is also implied in the requirement that the diagnostic pool contain particles more numerous than those belonging to a single entity. Table 1 shows a matrix of interparticle coefficients diagnostic of three entities after a process of rearrangement akin to factor or cluster analysis. If the particles are persons, then both primary and secondary group membership are indicated.

Using roll calls as the population of instances, such an approach was employed by Stuart Rice in his chapter on "The identification of blocs in small political bodies" (19), his index of agreement being the practical equivalent of a point correlation between legislators. As he indicates, the purely inductive process becomes overwhelming in magnitude as large numbers of particles are involved. Where a coefficient of common fate is employed as more than a heuristic referent, it will be most easily employed to test already present hypotheses about the location of group boundaries, or to decide which of two modes of classifying a specified population into groups is the more efficient. Applied thus, there are useful short-cut approximations available. If a mean position of all particles in an hypothesized group are computed for each sampled occasion, the variance of these means compared with the variances of the values for the individual particles generates an estimate of the average intercorrelation among the particles, on the basis of acceptable approximating assumptions (15, 17, 18). The mean values of hypothesized group 1 and the means of hypothesized group 2 can be correlated, and this correlation converted into an inferred inter-entity particle correlations, or group 1 and group 2 can be pooled and the inferred average intercorrelation of the composite compared with that of the separated parts, etc. To apply this approach to the composite common fate coefficient, each separate time-measure combination would be treated as a separate occasion, a separate group mean being computed for each.

Similarity as a source of boundaries

If we were upon a single occasion to describe the attributes of each molecule or person, one could in parallel fashion compute one (or many) coefficients of similarity. These could then be used to draw boundaries. Many of the boundaries would be sharp. But in the gravel pit, among the social insects when several colonies of the same species are pooled, and for human social groupings, the boundaries drawn by similarity seem somewhat secondary to those based upon common fate. Thus "red heads" are less a "real" group than "Negroes," in the sense that the latter boundary is confirmed by some degree of common fate, while the former usually is not (except in the one instance recorded by Watson [10].) But perhaps it is more important to emphasize that usually the boundaries concur, but that one criterion may draw a boundary where another does not. Thus common fate may not separate a stone from the spot of tar adhering to it (unless our sample of occasion includes a solvent bath)

while similarity does. Thus the piece of reinforced concrete is less "unified" than a piece of soft limestone by the coefficient of similarity, more unified by the coefficient of common fate, if pulverizing pressures are included in the sample of events.

Many specific procedures and coefficients are available for similarity, and the elaboration in practice has been much greater than for common fate, probably because of the practical difficulties of accumulating data extended in time. To begin with, one could take measures of particles on some single attribute, and then by a sorting procedure applied to the raw scores achieve groupings on this single dimension of similarity which might show relatively sharp boundaries, discontinuous distributions, or types. It must be stated, however, that where applied in this way in practice, single dimensional approaches have rarely discovered types or entities of any clear discreteness or multiple confirmability. On the other hand, starting from hypothesized boundaries or groupings, single dimensions may occasionally be discovered which are reasonably dependable diagnostic clues for entities confirmable by common fate or other diagnostic procedures. Analysis of variance is applicable in such cases. Uniforms, fraternity pins, and shibboleths approach this limiting case.

More common and more generally useful are composite similarity indices elaborated in psychology as indices of profile similarity (8), *Q*-type correlations, (7), etc. The initial use in the social sciences was probably in anthropology. Driver and Kroeber (11) have summarized the early usage and provided a number of applications of their own, trying out a variety of measures based upon the shared and nonshared presence and absence of specific culture traits. While they employed this technique to classify residentially localized social units into spatially dispersed culture areas or historically connected families, it would certainly have been applicable to the classifying of specific individuals, had that been a problem. Indeed, the procedure represents a formalization of the means by which any anthropologist would classify a single person, dressed in his native costume and interviewed about his customs, etc.

In the employment of dichotomous data and fourfold indices of similarity (as tetrachoric correlations, phi coefficients, percentage agreement or percentage of shared traits, etc.) there is a special problem not found when more refined measures of degree are used, having to do with the number of entries in the cell indicating traits absent in both cultures. The count in this cell can be expanded up to the limits of the investigator's ingenuity. Winer (25) has proposed a coefficient invariant in this respect, and Adkins (2) has applied it to a classification of groups. The more general problem of what traits to employ and how many highly similar traits to use probably has no general answer, each set being in the nature of a hypothesis. Once again, it is the relative level of intra-entity coefficients against the background of inter-entity coefficients that provides the boundary.

If a *Q*-correlation type of index is employed, and if extant hypotheses about grouping are being tested, then the procedure of inferring average interparticle correlations from average values for the particles of an hypothesized group is available, as for the common fate coefficients.

While in general, common fate may be found to be more central to the diagnosis of entities than similarity, there is often in practical diagnostic procedures an iteration between similarity and common fate criteria in which an observed similarity dimension may provide a hypothesized grouping which is then tested for intragroup homogeneity on various dimensions of common fate. This iteration makes possible the extension of common fate procedures into situations otherwise inaccessible. Note that the common fate procedures already suggested require the reidentifiability of the individual particles on successive occasions. This is an extremely difficult requirement to fulfill, and is unnecessary if we are testing the hypothesis that a similarity grouping shares common fate on other dimensions than the one used for classifying similarity. Thus even if the football players had no numbers on their backs, on the basis of the spatial

distribution at various moments of each uniform type one could diagnose the separate teams and their discreteness from each other and the onlookers. Analysis of variance techniques seem most appropriate here. The simultaneous handling of multiple-attribute common fate, as latitude, longitude, and elevation, may require extensions in the direction of covariance analysis or discriminant function analysis.

If one were to apply the ancient distinction between primary and secondary qualities, and stop short of regarding all diagnostic procedures as secondary, then similarity might be judged more secondary than common fate. But in the elaboration of diagnostic procedures as in the vertebrate eye and the procedures of science, similarity plays the larger role, and the traditional primary qualities the least elaborated role of all.

Proximity as a clue for entity

Contemporaneous spatial contiguity of particles seems still less "essential" than either common fate or similarity as a cue for entity.[3] However, the fact that it is used by the visual perceptual machinery indicates its general usefulness, and certainly the ethnologist finds it highly correlated with social entitativity, although Tryon's data (22) indicate some interesting exceptions for high social level groupings in the San Francisco Bay area.

Perhaps most significant is the employment of proximity to draw group boundaries where the distribution of interparticle spacings becomes large. Certainly boundaries so drawn would confirm most atomic, molecular, indigenous social group, and other entity diagnoses. For human groups, face-to-face communication processes made possible by proximity generate similarity and feelings of belongingness which make coordinated action and hence common fate more likely.

The value of proximity as a predictor of common fate is primarily a function of the kind of adhesives which hold the particles together in common fate. For atoms, molecules, and the run of inanimate physical objects including the solar system, these adhesives require adjacency. For modern social groups, this may not be essential. Thus the FBI may be a group with sharper boundaries, a harder, more solid social entity than is Davenport County, Iowa.

Reflection or resistance to intrusion of external energy, matter, or diagnostic probes

Still more basic to the visual machinery than the Gestalt organizational principles is diagnosis of boundaries through the reflection or alteration of the speed of particles of matter or wave energy (6). Thus we could locate the boundaries of the stone through the interruption, absorption, or slowing of electromagnetic waves of the light or heat frequencies, of acoustic air compression waves, of tossed ping-pong balls, or of probing fingers. The stone is "opaque" to a wide variety of waves and substances in motion. Its boundaries can thus be plotted, and would be highly coincident or multiply confirmed between the various procedures. It is here that the traditional distinction between primary and secondary qualities has most direct relevance. The primary qualities represent interruptions of the locomotion or manual probes of the diagnosing organisms. The secondary qualities represent probes of lower energy content (as light, sound, or radar waves) when these are employed so as to substitute (6) for the primary ones.

Analogously, group boundaries may occasionally be plotted by the sentry's challenge, by the deceleration locations in the traveler's locomotion, by indoctrination procedures, etc. Hemphill and Westie (16)

[3] Brunswik and Kamiya (5) have estimated it to have an ecological cue validity of only around .20, although their method of computation was different than that implied here which would probably in general give a higher value. Consider the space of an area as divided into cubic centimeter units. Taking pairs of units at random, ask if these are part of the same thing (as judged by all other available diagnostic clues) and how far apart are they. A biserial coefficient, or a coefficient of contingency could be based upon such data, relevant to the Brunswik and Kamiya question of cue validity. The absolute magnitude would, however, vary with the size of the units of reference chosen, the size of the entities in the area, and the breadth of the area sampled.

have suggested permeability as a clue for the rigidity of group formation. It remains to be seen whether practical diagnostic procedures using the reflection of probes will be usable at the group level.

Internal diffusion, transfer, communication

For certain inanimate entities, particularly those of metal, and for animate entities, boundaries can be plotted by diffusion limits and diffusion rate uniformities within the entity, or diffusion rate discontinuities between the entity and adjacent substance, as for heat or electrical potential within metals, and as for blood sugar and oxygen with animals. Thus by use of a radioactive tracer potassium introduced into an animal's food, one could plot the animal's body boundaries with a Geiger counter or photographic paper, plotting essentially the boundaries of internal substance transfer. Plotting the distribution of manufactured products or foodstuffs would similarly diagnose the boundaries of an economic system. Tracings of the boundaries of distribution of a given message have a similar logic. The communication rate between particles, boundaries implied by depressions in the interparticle transaction rate, etc. represent ways of plotting the boundaries of functional social units which Deutsch (9) in particular has emphasized.

At this point we are getting beyond the task of this paper, into Spencer's second question: if there are social groups that meet the criteria of being entities do they also meet the further, more restrictive and more complicated criteria of being organism-like entities? This problem has perhaps already been given adequate attention (e.g., 3, 12, 13, 20). It is to be hoped that such criteria always be understood as implying data series predictions confirmable in degree in any specific instance. As the problem becomes more complex, this operationalizing becomes more difficult. Note that in spite of the obviously greater rigidity of organization in the white rat as opposed to the stone, the latter has higher interparticle common fate and similarity coefficients, although the white rat would still be diagnosed as an entity by these sieves.

Illusions and multiply confirmed boundaries

In the diagnosis of middle-sized physical entities, the boundaries of the entity are multiply confirmed, with many if not all of the diagnostic procedures confirming each other. For the more "real" entities, the number of possible ways of confirming the boundaries is probably unlimited, and the more our knowledge expands, the more diagnostic means we have available. "Illusions" occur when confirmation is attempted and found lacking, when boundaries diagnosed by one means fail to show up by other expected checks. Most frequently, these illusions involve "superficial" similarity contours, as in two-dimensional paintings of "real" objects, distracting contours in camouflage, and the wearing of enemy uniforms in espionage.

In the typical arena of the anthropologist, it seems likely that a multiple confirmability of boundaries similar to that for biological and inanimate entities may be found. For the sociologist and political scientist, a preliminary scouting indicates that the redundancy of boundary diagnoses may be so considerably less and the number of empirically justified delineations of units open to him may be so considerably greater, that the common attitude of regarding all such delineations as arbitrary may seem justified. However, even here an emphasis on the potential multiple diagnosibility of boundaries seems important, even if the boundaries be more numerous, less sharp, and more overlapping, with multiple group membership being the rule. It might well be alleged that any scientifically useful boundary must be confirmable by at least two independent means. Such an emphasis seems necessary if sociology is to retain those attitudes of discovery, problem solving, independent confirmation and validation of construct which have characterized the successful sciences.

Taking an evolutionary perspective on the visual apparatus, it seems clear that the development was predicated upon an environment populated with stable, solid, clear-cut entities—that it could never have developed in a world of fuzzy-edged amoeboid clouds or in a completely fluid, homo-

geneous material space. By analogy, one might guess that the development of any science is predicated upon the discovery of such natural nodes of material organization, upon stable discontinuities. If discreteness and multiple-diagnosability of entities at the social level turns out upon examination to be lacking, then the possibility of a social science representing a separate level of analysis from the biological or psychological will be eliminated. Most sociologists and behavioral scientists judge from their preliminary scouting that while confusing or complex, the situation is not that bad, and that there is the possibility of a science of social groups per se. In the end, this is an empirical matter not to be decided on a priori grounds (4).

It can be noted that the diagnostic procedures here suggested all seem to depend upon the acceptance or achievement of an entity diagnosis at a lower level. Historically this seems not to be so, as we started with diagnosis of middle-sized entities and have worked down to the neutrinos and up to the solar system. In any event, this should provide no qualms for the social scientist who is certainly justified in basing his diagnosis of social entities upon an acceptance of the biologist's diagnosis of organisms. The behavioral scientist should certainly allow himself an applied epistomology which makes use of the achievements of the more advanced and/or more basic sciences, rather than limiting himself to what can be deductively justified on the basis of the contemplations of a mortal philosopher, sitting immobile with both eyes closed, his mind swept clean by intent at least of the biasing effects of all prior learning.

Consistent nominalism and the status of psychology's individuals as entities

In the so-called nominalists of Spencer's example and of Warriner's recent discussion (23) there is a basic inconsistency in the application of nominalism. The sociologist's entity constructions are treated as fictional while the psychologist's entity constructions, such as persons, individuals, or organisms are given a real status, rather than being treated nominalistically. Needless to say, this inconsistent nominalist is often a psychologist looking over the fence at his neighbor sociologist's area. Given the attunement of the anatomic diagnostic procedures to entities of the size of organisms, this inconsistent position can have great intuitive appeal.

Of course, even as diagnosed by the tedious actuarial procedures suggested by this paper, the biologist's organism of protoplasmic cells would turn out to be a much more discrete, clear-cut, hard, and multiply diagnosed entity than almost any of the sociologist's entities (unless the biologist happened to choose slime mold [13] as an example.) But this is the biologist's organism, not the psychologist's. The psychologist's organism would have to be diagnosed or confirmed by common fate and similarity coefficients among responses. Since responses lack reidentifiability, common fate considerations can only be employed to test the hypothesis that biological boundaries are also the boundaries of psychological entities. Analysis of variance procedures with the biological person as a classification criteria (17) are appropriate at this point. Is there variance among a population of responses collected at various times from various persons which is associated with the individual? This turns out to be equivalent empirically to the problem of the reliability coefficient of a psychological measure. These are often high, but occasionally get as low as zero, and uniformly tend to be lower as the persons under study are more homogeneous sociologically. Certainly the boundaries of the psychological person are fuzzy enough to give the psychologist cause for modesty in setting standards for sociological entities. Often enough one can predict responses better from knowing a person's social locus and immediate setting than one can from a knowledge of this biological person's responses in past situations.

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(Manuscript received August 8, 1957.)

Obscurity is not at all the same as unintelligibility. Obscurity is what happens when a writer undertakes a theme and method for which the reader is not sufficiently prepared. Unintelligibility is what happens when the writer undertakes a theme and method for which he himself is not sufficiently prepared.

—JOHN CIARDI

If you would rather have a vacation in Capri than a red sport car, and if you would rather have the car than a thirty-foot yawl, is it safe to assume that you would prefer the vacation to the boat? This assumption of "transitivity" is often made in theories of behavior, particularly in theories of decision making. But even though transitivity of preferences is a basic assumption of economic theory, several economists maintain that stable intransitive preferences do exist. You might well, they would say, take the yawl, not the vacation. Two experiments designed to test these assumptions show that intransitivity may be explained as a result of random choices among indifferent objects, and that the assumptions of transitivity still hold.

THE TRANSITIVITY OF PREFERENCES

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THE transitivity of preferences is a question of central importance to both psychology and economics. By transitive we mean that if A is preferred to B, and B is preferred to C, then A is preferred to C. The method of economic theory is often a formal method in that the theorists make assumptions and from these assumptions, derive theorems. One of the common assumptions of economic theory is that man is rational. The concept of rationality consists essentially of two parts. The first is that man is capable of a weak ordering of preferences, and the second is that man can choose to maximize something. We are concerned with the first of these two assumptions. This assumption also consists of two parts. The first part assumes that of two states, A and B, man is able to tell whether he prefers A to B or B to A or that he is indifferent between them. The second assumption is that the rational man's preferences are transitive, i.e., that if he prefers A to B and B to C, he will prefer A to C or if he is indifferent between A and B and if he is indifferent between B and C, he will be indifferent between A and C. The empirical existence of stable intransitivity or circular triads would challenge the usefulness of the above assumption to economics. By circular triad we mean that A is preferred to B and B is preferred to C and C is preferred to A. The importance of the question of transitivity is broader than just economic theory. Any theory of decision making whether psychological, sociological, economic, or political must deal with the question of transitivity. This experiment will deal with the transitivity of preferences.

There have been several studies (2, 3, 4, 5, 6, 7, 9, 11, 12, 13) which have dealt with the transitivity of preferences. All of the studies relevant to intransitivity known to this author will be reviewed below. Those studies which claim to have demonstrated intransitivity have used rather complicated stimuli.

May (8, 11) had each student of a class of 65 students make paired comparison choices among three girls as to which girl of each pair he would rather marry. Each girl was described by three adjectives. The following is the description of the three girls: Girl 1—very intelligent, plain looking, and well off; Girl 2—intelligent, very good looking, and poor; Girl 3—fairly intelligent, good looking, and rich. Judgments of indifference were not permitted. It is to be noted that in the case of judgments among

[1] I am greatly indebted to Drs. L. Tucker and H. Gulliksen, who rendered invaluable assistance in the conception, performance, and report of these experiments.

three stimuli, there are eight possible configurations of responses as listed below.

1. $A > B; B > C; A > C$
2. $A > B; B > C; C > A$
3. $A > B; C > B; A > C$
4. $A > B; C > B; C > A$
5. $B > A; B > C; A > C$
6. $B > A; B > C; C > A$
7. $B > A; C > B; A > C$
8. $B > A; C > B; C > A$

("$A > B$" means A is preferred to B)

The second and the seventh of the above triads are circular. All the rest are transitive. If judgments are made at random, each triad of choices would be expected equally often. In this case, one-fourth (that is, two out of eight) of the cases might be expected to be intransitive. May (11) found that 17 out of his 65 students gave intransitive responses. This result is quite close to the one out of four ratio expected on a random judgment basis.

Papandreou (6, 13) performed the following experiments: He prepared triplets of bundles of hypothetical tickets of admission to plays, concerts, athletic events, etc. His subjects were asked to choose between pairs of bundles. Each bundle contained a total of four admissions to two different types of events, e.g., three concerts and one play. In the main experiment each bundle was compared with two other bundles containing the same sort of event. For every triplet of bundles the subjects made three choices because they were choosing between pairs of bundles. In an auxiliary experiment each bundle was compared to two other bundles which had different sorts of events in them so none of the bundles had the same sorts of events in common with the other two bundles in the triplet. For each pair of bundles the subject could give one of three judgments; bundle A is preferred to bundle B, or bundle B is preferred to bundle A, or the subject could judge he was indifferent between bundle A and bundle B. There are therefore 27 possible configurations of choices of which 13 are transitive. (In Table 1 all 27 triads are represented.) In the main experiment 5% of the choices were intransitive and in the auxiliary experiment 4% were intransitive. Papandreou concludes that transitivity exists for his specific experimental conditions.

Edwards (2, 3, 4, 5, 6, 7) conducted a series of experiments on probability preference. That is, Edwards has demonstrated that bets with certain probabilities of winning are preferred to bets with other probabilities of winning. All of Edwards' experiments were of essentially the same design. The subjects were asked to choose between pairs of bets according to the method of paired comparisons. Some of the bets had positive expected values, (i.e., the subject would be expected to win in the long run), some had negative expected values, and some had zero expected value. In the main experiment only bets with the same expected value were paired so that there was no objective reason to expect that one bet would be logically better than another. The subjects made their choices among bets under three conditions: (1) they imagined they were betting, (2) betting for worthless chips, and (3) betting for real money. The results under each of these conditions were similar.

Edwards feels that the results indicated that there were two factors which were im-

TABLE 1

ALL POSSIBLE CONFIGURATIONS OF TRIADS WHEN INDIFFERENCE JUDGMENTS ARE PERMITTED

("AIB" means the subject is indifferent between A and B. ^{t} means transitive and ^{c} means circular.)

1. $A>B$: $B>C$: $A>C^t$	
2. $A>B$: $B>C$: $C>A^c$	
3. $A>B$: $C>B$: $A>C^t$	
4. $A>B$: $C>B$: $C>A^t$	21. $A>B$: BIC: AIC^c
5. $B>A$: $B>C$: $A>C^t$	22. $B>A$: BIC: AIC^c
6. $B>A$: $B>C$: $C>A^t$	
7. $B>A$: $C>B$: $A>C^c$	23. AIB: $B>C$: AIC^c
8. $B>A$: $C>B$: $C>A^t$	24. AIB: $C>B$: AIC^c
	25. AIB: BIC: $A>C^c$
9. AIB: $B>C$: $A>C^t$	26. AIB: BIC: $C>A^c$
10. AIB: $B>C$: $C>A^c$	
11. AIB: $C>B$: $A>C^c$	27. AIB: BIC: AIC^c
12. AIB: $C>B$: $C>A^t$	
13. $A>B$: BIC: $A>C^t$	
14. $A>B$: BIC: $C>A^c$	
15. $B>A$: BIC: $A>C^c$	
16. $B>A$: BIC: $C>A^t$	
17. $A>B$: $B>C$: AIC^c	
18. $A>B$: $B>C$: AIC^t	
19. $B>A$: $B>C$: AIC^t	
20. $B>A$: $C>B$: AIB^c	

portant in determining choices between bets: (1) preference or dislike of risk taking and (2) specific preferences among probabilities. Edwards gives as examples of the former the fact that some subjects preferred a low probability of winning a large amount to a high probability of winning a small amount of money. He gives as an example of the second factor the fact that subjects consistently preferred bets involving a 4/8 probability of winning to all other bets with a positive expected value and the subjects avoided bets with 6/8 probability of winning. These results were independent of the amount of money involved as long as there was no difference in the expected value of the bets. Edwards did these experiments with a number of slightly different experimental conditions with different subjects and found that the different conditions yielded approximately the same results. Circular triads occurred in Edwards' first experiment (3) in about 20% of the total number of times they could occur. Edwards feels the subjects learn to be consistent as they gain experience with the choices they must make. Edwards says that although circular triads might be expected to occur if the subjects were indifferent, the results of the vote count show that they did not choose at random. This argument of Edwards is logically fallacious. Edwards' logical fallacy is essentially the fallacy of the undistributed middle. It is logically possible that the significant agreement between subjects could occur in those 80% of the possible triads where the subjects were consistent. (This can be shown using Kendall's formula [10, p. 121, 11.1].) The Kendall coefficient of consistency resulting from agreement on 80% of the triads is approximately equal to that obtained from Edwards' data. That is, it does not follow from the premises that if there is significant agreement between subject choices and that there are circular triads in 20% of the possible triads of these choices, that there is agreement on the circular triads among subjects. The vote count showed that the subject did not choose at random for all the choices. They could have chosen at random on some of the choices, i.e., those choices which resulted in circular triads.

It is relevant at this point to explore some of the logical possibilities for transitivity and intransitivity of choice. A subject can be asked to make choices at different times. If a subject always prefers A to B even though he is asked which he prefers many times, one can say that the probability that he will prefer A to B is one. Similarly if he is indifferent between A and B and he is forced to make a choice many times, the probability that he would prefer A to B would be .5. If a subject has a slight preference of A over B, the probability that he prefers A over B might be .6. Let us consider the implications on transitivity of these probabilities.

Case 1

$P(A > B) = 1$
$P(B > C) = 1$
$P(A > C) = 1$

(the notation "$P(A > B) = 1$" reads the probability that A is preferred to B is one)
In this case, all triads would be transitive.

Case 2

$P(A > B) = 5$
$P(B > C) = 5$
$P(A > C) = 5$

In this case, circular triads would be expected in two out of the eight possible triads.

Case 3

$P(A > B) = 6$
$P(B > C) = 7$
$P(A > C) = 8$

If the probabilities existed, the following triads would be expected with the following probabilities of occurrence assuming the joint occurrences are independent.

	$A > B;\ B > C;\ A > C = 0.336$
	$A > B;\ B > C;\ C > A = 0.084$
	$A > B;\ C > B;\ A > C = 0.144$
Triads 2 and	$A > B;\ C > B;\ C > A = 0.036$
7 are circular.	$B > A;\ B > C;\ A > C = 0.224$
	$B > A;\ B > C;\ C > A = 0.056$
	$B > A;\ C > B;\ C > A = 0.096$
	$B > A;\ C > B;\ C > A = 0.024$

Thus in Case 3 the choices are not made entirely at random since one can (in the $A > C$ case for example) predict that the subject will prefer A to C eight out of ten times. However, one cannot predict on which trials the subject will prefer C to A, so there is in some sense some random element in the choices. The subject is, in some sense, choosing with a bias on chance. Case 3 offers a possible explanation for the data of May and Edwards.

Flood (8, 9) asked subjects for prefer-

ences between objects such as physical objects and money. He found circular triads. May (11) reports a study by McCulloch (12) which found circular triad in a rat experiment where rats preferred food to sex, sex to avoidance of pain, and avoidance of pain to food. Upon tracing the references no detailed report of the experiment could be found so no evaluation of the experiment can be made. The above summary covers all the evidence on intransitivity known to this author.

Thus intransitivity of preference has been asserted (2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13) and theories about intransitivity built (11). A certain amount of intransitivity in data can be explained as a result of random choice between indifferent objects. The problem is as follows: *Are circular triads explainable as a result of random choices among indifferent objects or do stable intransitivities exist?* Circular triads occurring in less than 25% of the possible triads can always be explained as a result of random choice among indifferent objects. Therefore, one way to determine whether the circular triads are stable or a result of chance is to present the same subject with the same stimuli on two different occasions. If the triads are stable, the same circular triads will be circular on both presentations of the stimuli. However, if the triads are a result of random choice among indifferent objects only a few of the circular triads would be circular on both presentations. *In none of the above studies were the same triads presented to the same subjects on two different occasions. Therefore the above evidence fails to demonstrate that stable circular triads exist.*

The purpose of the present experiments is to obtain evidence concerning the stability of intransitivity of judgments. In order to accomplish this it is clearly necessary to obtain responses of the same subjects upon two different occasions to the identical stimuli. The question then becomes whether the same people make the identical circular judgment on the two occasions, to a greater extent than one would expect on a random basis. In Experiment I stimuli similar, but not identical with May's stimuli, were used. In Experiment II, a situation similar, but not identical with Edwards' experiment, was used.

EXPERIMENT I

Method

We presented a paired comparisons schedule of nine stimuli to 47 undergraduate male students in an elementary psychology course. Our stimuli were verbal descriptions of girls. These verbal descriptions were typed on lantern slides and shown to the students with a slide projector. Each girl was described by three adjectives. The descriptions of the girls were as follows: (1) plain, very charming, wealthy; (2) pretty, average charm, wealthy; (3) pretty, very charming, average income; (4) pretty, charming, well-to-do; (5) average "looks," charming, wealthy; (6) average "looks," very charming, well-to-do; (7) beautiful, very charming, poor; (8) beautiful, average charm, well-to-do; (9) beautiful, charming, average income. Each stimulus was compared with every other stimulus. Thus there was a total of 36 comparisons. The students were asked to indicate which of the two girls in each comparison they would rather marry. The study was repeated with the same students using the same stimuli six weeks after the original presentation.

The students took considerable time making their choices and seemed to be making their choices carefully and honestly.

Results

It was found that 22.8% of the preference choices changed from presentation to presentation and 77.2% of the preferences stayed the same. All of the circular triads of all 47 students were counted. The results are presented in the table below:

		SECOND	
		CT	Tra
FIRST	CT	28	173
	Tra	286	3461

CT stands for circular triads and Tra stands for transitive triads. The top notation refers to the second presentation and the side to the first presentation. Thus the CT-CT cell would be those triads which were circular on both presentations and the CT-Tra cell in column Tra and row CT would be those

circular on the first presentation but transitive on the second. The 3461 triads in cell Tra-Tra are transitive on both presentations but they are not necessarily transitive in the same direction. There are 1410 circular triads possible according to Kendall's formula (8, p. 133, 12.3) for the number of circular triads possible. The number of repeated circular triads is small compared with this number (28 to 1410). (30 circular triads is the maximum number of circular triads in a 9 x 9 table; 30 x 47 [subjects] is 1410.) It should also be noted that there were many triads (173 and 286) which were circular on one presentation and not the other. So the number of repeated triads is small compared to the possible number of circular triads and to the number of circular triads which are circular on one presentation and not on the other. Kendall (8, p. 124, 11.2) has a chi-square test to see if the number of circular triads could be explained by assuming the subjects were choosing at random. The number of circular triads which each student made on each presentation was tested and in all cases the chi square was significant. One can therefore reject the hypothesis that the choices were made at random.

One should note that most of the triads are transitive. One should also note that the sum of the frequencies in cells CT-CT, CT-Tra, and Tra-CT cannot possibly exceed 1410 and the frequency in cell Tra-Tra cannot be less than 2538.

Of the 28 repeated circular triads, 16 are circular with the same preferences expressed on both presentations and 12 are circular with reversed preferences on different presentations, i.e., $A > B; B > C; C > A$ and $B > A; C > B; A > C$. By stable circular triad is meant circular triads which have the same preferences expressed on both presentations. However, if the subjects were choosing at random one would expect the number of stable circular triads to be the same as the number of reversed circular triads. A chi-square test was performed to see if the observed data differed significantly from the number expected if the subjects were choosing at random.

	stable	reversed	
observed	16	12	chi square = .57
theoretical	14	14	P = .46

Therefore, the observed data do not differ significantly from the case in which the subjects were choosing at random.

To sum up the discussion so far, there are many transitive triads and very few stable circular triads, and there are almost as many reversed circular triads as stable circular triads which would indicate that on those triads which were circular, the subjects were choosing at random. Thus we find no evidence for the existence of stable circular triads.

A chi-square test was performed using the experimentally determined marginal frequencies to predict the theoretical cell frequencies.

	CT	Tra	
CT	28	173	201
Tra	286	3461	3747
	314	3634	3948

Chi square was equal to 10.3, which is significant. If we convert to a phi coefficient (1, p. 382, 18.29) we get a phi coefficient of 0.05.

Since this finding is contrary to the other findings of this study, we investigated the possible source of the apparent increase in the number of repeated circular triads. One explanation can be found in the heterogeneity of the popularity of the different objects of the choices. For example, consider two triads, one on stimuli A, B, and C and the other on stimuli D, E, and F. On the first triads the subjects were choosing at random. ($P(A > B) = .5$ means that the probability of preferring A to B is .5.)

		CT	Tra
$P(A > B) = .5$	CT	.0625	.1875
$P(B > C) = .5$	Tra	.1875	.5625
$P(A > C) = .5$			

On the second triads the subjects are choosing with a bias on chance.

		CT	Tra
$P(D > E) = .9$	CT	.0081	.0819
$P(E > F) = .9$	Tra	.0819	.8281
$P(D > F) = .9$			

If we combine these two tables we get the following table:

	CT	Tra
CT	.0353	.1347
Tra	.1347	.6953

One will note that in this result the CT-Tra and Tra-CT cell frequencies are smaller than would be expected if the marginal frequencies were used to calculate the theoretical cell frequencies. This result is similar to the above table of observed frequencies. We would conclude that the sources of this apparent non-randomness are in the heterogeneity of the popularity of different objects of the choices.

Discussion and conclusion

A reasonable conclusion would be that the probability of the preferences on some of our choices was fairly high and on triads involving these choices the subjects had transitive triads with the preferences the same on both presentations. However, there were other choices where the probabilities of the preferences were low, hence, there were some circular triads. We know of no evidence that there are stable circular triads. We would explain the fact that there are more circular triads in the second presentation than the first by assuming that the subjects tried harder on the first presentation to state their preferences. Therefore, we would conclude:

1. The majority of the preferences are consistent.

2. There is some inconsistency which can be explained as a result of random choices among indifferent objects. The tests of significance are consistent with the notion that the circular triads are a result of choices of subjects in which there is a random element. Evidence on the stability of circular triads is essential before one needs to deal with problems of inconsistent preferences. This study illustrates one way in which data may be obtained and analyzed to check on the stability of circular triads.

EXPERIMENT II

It could be argued that the type of judgment required in Edwards' experiment is so different from that required in May's experiment that stable circular triads could occur in one situation and not the other. The purpose of this experiment is to obtain evidence in a situation similar to the one used by Edwards. The question is: Do circular triads result from random choices among indifferent objects or do stable intransitives exist?

Method

This experiment is similar in design to the "just imagine" sessions of Edwards' main experiment (3). The subjects were 24 Princeton undergraduates. The experimental material was 15 pairs of bets typewritten on sheets of paper. The bets were similar to the bets of zero expected value used by Edwards, who stated his bets in terms of certain scores made on a pinball machine. For example, one of Edwards' bets is as follows: "if you roll a 2, a 4, a 7, or an 8 you win \$1.05. If you roll anything else (a 1, a 3, a 5, or a 6) you lose \$1.05." The bets in this experiment were stated in the following form; "bet \$1.05 to win \$1.05 with the probability of winning one out of two." Thus although the wording of the bets in this experiment is different from the wording used by Edwards, the amount of money won or lost and the probability of winning is the same. Each of the eight different bets is similar to one of the eight bets used by Edwards. These bets were used to make the 15 pairs of bets presented to the subjects. Each bet appears in at least one triad. There are 12 circular triads possible. The pairs of bets used were those most likely to produce circular triads. All the bets have zero expected value (i.e., the expectancy of winning or losing in all the bets is zero) so there is no logical reason for preferring one bet to another bet. Table 2 lists all the pairs of bets.

The subjects were asked to imagine they were betting and to check which bet they preferred to bet of each pair of bets. Those subjects who made circular triads on the first presentation filled out the schedule of 15 identical comparisons two weeks after the first presentation.

Results

Seventeen of the 24 subjects were consistent. Seven of the subjects made a total of 11 circular triads. These subjects were asked to fill out the schedule of bets two weeks after the first presentation. On this

TABLE 2

1. bet \$0.84 to win \$1.40 at 3/8 bet \$2.10 to win \$0.70 at 3/4	8. bet \$4.20 to win \$0.60 at 7/8 no bet
2. bet \$1.40 to win \$0.84 at 5/8 bet \$4.20 to win \$0.60 at 7/8	9. bet \$0.70 to win \$2.10 at 2/8 no bet
3. bet \$0.60 to win \$4.20 at 1/8 bet \$0.70 to win \$2.10 at 1/4	10. bet \$0.84 to win \$1.40 at 3/8 bet \$1.05 to win \$1.05 at 1/2
4. bet \$1.40 to win \$0.84 at 5/8 bet \$2.10 to win \$0.70 at 3/4	11. bet \$0.84 to win \$1.40 at 3/8 bet \$4.20 to win \$0.60 at 7/8
5. bet \$0.60 to win \$4.20 at 1/8 no bet	12. bet \$1.05 to win \$1.05 at 1/2 bet \$1.40 to win \$0.84 at 5/8
6. bet \$2.10 to win \$0.70 at 6/8 bet \$4.20 to win \$0.60 at 7/8	13. bet \$1.40 to win \$0.84 at 5/8 no bet
7. bet \$2.10 to win \$0.70 at 3/4 no bet	14. bet \$0.84 to win \$1.40 at 3/8 no bet
	15. bet \$0.84 to win \$1.40 at 3/8 bet \$1.40 to win \$0.84 at 5/8

second presentation, of the 11 triads circular on the first presentation, seven were consistent, three were circular in the same direction, and one was circular in the reversed direction. If the subjects were choosing at random among indifferent objects, you would expect that for every circular triad in the same direction, there would be one circular in the opposite direction. The observed data do not differ significantly from chance values. In fact you would expect by chance that a discrepancy from the above theoretical values greater than or equal to the observed discrepancy would occur with a probability of 5/8. Therefore, these circular triads can be explained by the assumption that they are the result of random choice among indifferent objects.

Discussion

Far fewer circular triads are found here than in Edwards' experiment. One reason could be that Edwards' subjects took four experimental sessions to make their paired comparisons judgments. Therefore, the subjects would have sometimes judged one of the comparisons at a different session than he judged the other comparisons of a triad. In the time between sessions the subject could have changed his mind about his preference.

Although fewer circular triads were made in this experiment than in Edwards' experiment, the fact still remains it has been shown in this experiment that using stimuli similar to Edwards no evidence for the existence of stable circular triads was found.

CONCLUSION

The intransitivity of preferences has been asserted in previous studies. It has been shown that a certain amount of intransitivity can be explained by random choices among indifferent objects. A review of all the previous studies on intransitivity showed there was no conclusive evidence for the existence of stable circular triads. Two experiments were performed using similar stimuli to those used in previous studies. The tests of significance of the experimental data are consistent with the notion that the circular triads found are a result of choices of subject in which there is a random element. Therefore, no evidence has been found for the existence of stable circular triads. Evidence on the stability of circular triads is essential before one needs deal with the probability of intransitive preferences. This study illustrates one way in which data may be obtained and analyzed to check on the stability of circular triads.

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(Manuscript received July 5, 1957.)

A rock is nothing else than a society of molecules, indulging in every species of activity open to molecules. I draw attention to this lowly form of society in order to dispel the notion that social life is a peculiarity of the higher organisms. The contrary is the case. So far as survival value is concerned, a piece of rock, with its past history of some eight hundred millions of years, far outstrips the short span attained by any nation. The emergence of life is better conceived as a bid for freedom on the part of organisms, a bid for a certain independence of individuality with self-interests and activities not to be construed purely in terms of environmental obligations. The immediate effect of this emergence of sensitive individuality has been to reduce the term of life for societies from hundreds of millions of years to hundreds of years, or even to scores of years.

—A. N. WHITEHEAD

It is probably safe to say that there is nothing that has not been done to try to help people who have what is called "mental illness"—soft music and scalding in hot water, snake pits and electric current through the brain, stinging showers, starvation, milk diets, tranquilizing drugs and free association and casting out of devils and good hard work—but none can yet be said to be truly effective. Psychotherapy, especially since the days of Freud, has come to mean a treatment that is made up mainly of talk, a verbal exchange between the patient and the person who is trying to help him. With the development of this kind of treatment, it has been increasingly recognized that psychotherapy is characterized not only by its techniques, but also by the personal relationship between the patient and his therapist. This being so, the therapist himself, his background, attitudes, experiences, and personality must be put under the microscope for careful scrutiny and analysis if valid knowledge about how to treat mental illness is to be obtained.

THE PSYCHOTHERAPIST'S CONTRIBUTION TO THE TREATMENT PROCESS[1]

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RESEARCH on psychotherapy may be focused on three elements: the therapist, the patient, and the therapist-patient interaction. This investigation is concerned with the therapist's contribution to the treatment process—what the therapist *does*, and how his procedures are related to and influenced by his background, training, experience, and personality. Research on the therapist's contribution is not, of course, any more important than research on the personality processes which psychotherapy seeks to influence, but it is a necessary link in improving our understanding of what psychotherapy is about.

The performance of any two therapists, even in clearly defined and highly specific situations, will reveal differences. It is the antecedents of such differences that constitute the focus of our inquiry. By the same token, there will be many similarities, which will be equally important.

Systematic differences in performance may be a function of the therapist's training, experience, and personality—to name a few

[1] This investigation was supported by a research grant (M-965) from the National Institute of Mental Health, U. S. Public Health Service. Grateful acknowledgment is made to Winfred Overholser, M. D., Executive Officer of the Department of Psychiatry, under whose general supervision this work was carried out. To Leon Yochelson, M. D., consultant to the project, I am indebted not only for many constructive criticisms, suggestions, and comments but also for his unwavering support and friendly encouragement. It is also a pleasure to acknowledge the contributions of my research associates, Mrs. Rebecca E. Rieger and Mrs. Louisa R. Bilon. I wish to thank my friends, Drs. Dorothy E. Green and J. B. Chassan, for valuable advice on statistical problems.

The wholehearted collaboration of the following organizations is sincerely appreciated: Saint Elizabeths Hospital; Johns Hopkins Hospital; Maryland Psychiatric Society; Washington Psychiatric Society; Postgraduate Center for Psychotherapy; Veterans Administration Hospital, Perry Point, Md.; Michael Reese Hospital; and the Chicago Institute for Psychoanalysis. My debt to them is second only to that owed to the therapists who donated valuable professional time to this project. Without their interest, earnest participation, and patience, the work could not have been done.

[2] This research was done while the author was affiliated with the George Washington University School of Medicine.

of the more important variables. The effects may be relatively independent of the particular interpersonal situation (for example, an inexperienced therapist may not interpret at all or too much irrespective of the patient or the situation), or the effects may be highly specific (a therapist may respond to a patient's expression of hostility if it touches, say, his professional status but he may passively accept most other forms of overt hostility). In any case, it is clear that therapeutic techniques are not applied *in vacuo*, and that they are differentially affected by factors in the therapist's personality. His performance is determined—in part, at least—by the way in which he perceives the patient's behavior, interprets its meaning in the framework of his clinical experience *and* his own personality, and the way in which this meaning is reflected in his response. It is one of the peculiarities of the therapeutic situation that the therapist's interpersonal perceptions are immediately translated into action deliberately designed to effect a change in the patient's perceptions and behavior.

This investigation attempts to explore how the therapist structures the therapeutic problems (perceptions and evaluations), how these conceptualizations are related to what he proposes to do (treatment plans, goals, proposed procedures), and what he actually does (technique). Our aim is to investigate these interrelationships as well as to explain possible differences among therapists in terms of systematic effects produced by their training, experience, and personalities.

METHODS AND PROCEDURE

If we are interested in comparing the therapeutic behavior of one therapist with that of another, and if we want to study similarities or differences between groups of therapists, we must be able to make specific statements about the conditions which evoke this behavior. This is a basic requirement of any scientific investigation, which it has been extraordinarily difficult to approximate in psychotherapy research. It is impossible to have more than one therapist conduct an interview with the same patient at the same time, nor are we likely to succeed in selecting "comparable" patients. It follows that we have to compromise. One compromise that has often proven fruitful in science is to sacrifice some of the "realism" of the situation in exchange for improved experimental control.

To study the performance of psychotherapists under comparable controlled conditions we decided to use a sound film of an initial interview, to which subjects responded as vicarious interviewers. The respondents (therapists in the audience) were instructed to behave as if they were interviewing the patient. The major difference between a "real" interview and the experimental situation is the fact that the interaction is not between the audience therapist and the patient but between the film therapist and the patient. Consequently, the "interventions" of the audience therapist have no effect upon the patient or the course of the interview. Furthermore, the audience therapist's response is not exclusively to the patient, but rather to the totality of the patient-therapist situation as portrayed on the screen. This investigation, then, rests on the assumption that the audience therapist's simulated interviewer behavior bears a relationship to his performance as a therapist in similar real-life therapy situations, and that valid inferences can be drawn from this sample of his behavior. It is not maintained that the interview between the film patient and any audience therapist would have proceeded exactly as indicated by the therapist's hypothetical responses.

The film, part of a series produced by the Veterans Administration for training purposes, was originally entitled "A Clinical Picture of Claustrophobia." A 30-minute film, it has been described by its producers as an unrehearsed interview from which no significant parts were deleted.[3]

In the film the patient, a middle-aged man, complains of anxiety and gastrointestinal symptoms which appear to be of psychogenic origin. He relates an episode on a crowded street car in which he experienced an intense panic reaction; subsequently, he was unable to go out of the house

[3] The "Psychotherapeutic Interviewing Series" was directed by Dr. Jacob Finesinger and Dr. Florence Powdermaker. The film was part V of the series and was filmed in 1952. Grateful acknowledgment is made to Dr. Harvey J. Tompkins, Director, Psychiatry and Neurology Service, of the Veterans Administration, for granting permission to adapt the film for experimental purposes.

unless he was accompanied by his wife who was an invalid. He describes an intense conflict with his mother who emerges as a domineering and overprotective woman, and speaks at length about his "bad luck" in all phases of his life—his father died at an early age, leaving him with family responsibilities which were not shared by his brother—concluding that "I had three strikes against me before I even came to the plate." He speaks freely and his anger and resentment build up. His demanding attitudes come clearly to the fore, culminating in the statement—only half doubted—that the world owes him a living. While it is not clear that he has had psychotherapy before, he makes mention of previous interviews. He speaks of having obtained some "insight" but the fabric of his rationalizations and projections of blame belie this. The interview ends somewhat abruptly after the patient has made a passionate plea for help, couched in an intense demand for reassurance.

The therapist in the film is a young, inexperienced resident, who uses Finesinger's (1) technique of minimal activity. Essentially, he lets the patient tell his own story, frequently repeating a word or phase, and otherwise focusing very little. At times, he asks an informational question which seems to have the effect of diverting the patient from an emotionally charged topic. The therapist's attitude may be described as benevolent but rather distant; he is quite passive and offers no reassurance. The abrupt ending leaves it unclear how the interview was terminated or whether any treatment plans were formulated.

Here are a few highly condensed excerpts from the sequence:

P.: I have always been afraid . . . (T.: Mm.) . . . because (*laughs*) my association with my mother just made me scared to death. I have always been anxious and my association with her the first 20 years of my life has caused my marked anxiety . . . (T.: Mmm.) . . . and from 20 years from then up to now economic conditions have also caused it.

T.: You say the streetcar swayed?

. . . .

P.: I just never liked to . . to . . to be in a position where I could not throw my arms and get out if I wanted to, and this was one [being hemmed in by a big woman on a street car] that I could not get out fast enough . . . and it caused this panic reaction. (T.: Mmm.) And when I finally got off . . . it took two blocks to regain my composure, and then I went on to work.

T.: You felt hemmed in by this woman there?

. . . .

P.: And now the heart action has decreased, and now my trouble, doctor, is I am dizzy.

T.: Huh . . .

. . . .

P.: [Being hemmed in] causes a very uncomfortable, smothering sensation in my chest to be hemmed in; it just feels like maybe somebody is sitting on it . . . and then it keeps my stomach turning, quivering all the time . . . and it just makes me so anxious . . . (T.: Uh.) . . . to the point where I become overwhelmed with the anxiety by thinking about it and then I bring on this sensation, and then the sensations bring on my fear . . . (T.: Mmm.) . . . and then I just keep myself in a circle.

T.: Like someone sitting and twisting . . .

. . . .

P.: . . . so you see, all my anxieties stem from my economic worries . . . (T.: Mmm.) . . . and of course I developed a lot of antisocial attitudes along the way.

T.: You say your mother was with you in W——— for three years?

. . . .

P.: . . . after all, regardless of how many things that I think she has done that I don't like, that caused me difficulty, after all, she is still my mother; we can't get away from that.

T.: How? How is that?

. . . .

P.: I want to get well, so I get the hell out of this thing.

T.: You say, anxiety in association with your mother?

. . . .

P.: . . . every association that I've had has wound up unpleasantly. Even my work. Either I'd get fired or else I'd quit. So I have just come to the conclusion that I started out with three strikes against me before I came to the plate. It looks like it.

T.: What do you mean, "three strikes"?

. . . .

P.: I said to myself, "Just why the hell was all this handed to me? Why did I, why was I . . . why couldn't it have been Bill Smith over there?" . . . [it]makes me emotional . . . fill up even if I think about it . . . just feel like you want to bawl, cry . . . always had something to cause me to live some other way and it has been economic.

T.: Like you want to bawl, huh?

. . . .

P.: But, getting back to the sensations, that's the bugaboo . . . Now I have asked every doctor I have seen the same question . . . These sensations have been so horrible, and they're attacking me so viciously that . . . are they undermining my nervous system, are they going to cause me to have a heart disease . . .? That's all I want to ask you. Are they going to kill me? Will they cause me to have . . . will they cause my heart to flop out from under me some day or will they damage my nerve tissue or even my . . . my thinking? (End of film)

These quotations convey something of the patient's personality, his attitudes, and his approach to the therapist. The therapist's interventions, which are typical of his behavior throughout the interview, are minimal, noncommittal, and marked by uncertainty. It was thought that this factor tended to minimize possible competition on the part of the audience therapists.

To prepare the film for "audience participation," the interview sequence was interrupted at a number of predetermined points, giving the audience therapist an opportunity to indicate what he would have done had he been the interviewer. After numerous try-outs, 28 points of interruption were selected from an originally much larger number. They usually occurred immediately *before* an intervention by the film therapist (never after), at natural "breaks" in the patient's recital following the recounting of what might be potentially "significant" dynamic material, and at points immediately preceding a change of topic introduced by the film therapist. Certain points were selected on the basis of intuitive hunches. Finally, an attempt was made to obtain a fair sample of the respondent's therapeutic behavior without excessive interruption of the film sequence.[4]

Choice points were identified by a uniform title (WHAT WOULD YOU DO?) and a number from 1 to 28. An appropriate number of frames was inserted into the film sequence so that each title appeared on the screen for 30 seconds at the normal projection speed. The time limit was kept to a predetermined optimum to allow the respondent a reasonable amount of time, sufficient for most therapists, to consider and write down his comments. Nevertheless, the time limit set a pace which precluded lengthy deliberations. The experimental version of the film ran for approximately 50 minutes, including pauses. Once the operator had started the projector, the film ran continuously to insure complete uniformity for each presentation.

[4] I wish to thank the following psychiatrists for their helpful comments on this phase: Drs. Jay L. Hoffman, Leon Salzman, Norman Taub, Otto A. Will, Jr., and Leon Yochelson. The responsibility, of course, remains mine.

The film was shown to groups ranging in size from less than ten to well over one hundred. Instructions were presented orally by the investigator. The purpose of the experiment was briefly described, with emphasis on the comparative study of therapeutic techniques. Therapists in the audience were then requested to assume the role of a therapist with the patient in the film and to record their comments—what the patient would hear or see—on an answer sheet. They were cautioned that a pause in the film sequence was not to be construed as a requirement to respond, but that silences or other nonverbal communications were perfectly acceptable. The audience was also informed that this was a first interview, and that the film therapist knew no more about the patient than the respondent. (Some therapists criticized the investigator for not pointing out that the patient had had previous contact with psychiatrists, a fact which emerged as the interview unfolded.) The problem was simply presented as a situation in which a patient with an emotional problem approaches a professional person for help ("How would you help him?"). The instructions further stated that the therapist should assume that he had agreed to see the patient just once. It was the therapist's decision whether he wanted to treat him or make some other disposition. No guidance was given as to the kind of interview to be conducted (diagnostic, therapeutic, etc.).

Following the film showing, therapists were requested to complete a comprehensive questionnaire on diagnostic impressions, treatment plans and goals, formulations of the patient's dynamics, problems in treatment, estimates of the patient's anxiety, emotional maturity, social adjustment, prognosis with and without therapy, the respondent's attitude toward the patient and the therapist in the film, and an evaluation of the latter's performance. Appended to the questionnaire was a biographical information blank containing 24 questions about the therapist's training, theoretical orientation, and current therapeutic activities. The entire experimental procedure usually required approximately two hours.

The experimental procedure yielded two kinds of data: comments directed to the patient portrayed in the film (techniques) and responses to the questionnaire. The second group may be subdivided into diagnostic impressions and evaluations, treatment plans and goals, including technique problems, and biographical data which served as reference variables.

The sample

Complete data were collected from 237 therapists representing these professional groups:

Psychiatrists (including 32 analysts)	91	
Residents in psychiatry	43	
Total (medical training)		134
Psychologists		79
Psychiatric social workers		17
Others		7
Total sample		237

This paper will include primarily the responses of therapists whose training had been in medicine ($N = 134$).

Statistics on age, experience, and personal analysis are presented in Table 1. Additional information may be gleaned from the following distributions:

Status of psychoanalytic training:

	N	%
Completed psychoanalytic training (Member of the American Psychoanalytic Association, $N=21$)	33	25
In training	18	13
Plans	25	19
No psychoanalytic training	47	35
No answer	11	8

Status of personal analysis:

	N	%
Completed personal analysis	49	37
Presently undergoing analysis	39	29
Plans	15	11
No personal analysis	25	19
No answer	6	5

Amount of training in psychoanalytic principles and techniques (self-estimate):

	N	%
None	13	10
Very little	27	20
A fair amount	54	40
A great deal	34	25
No answer	6	5

Amount of experience in psychotherapy, not counting personal analysis (self-estimate):

	N	%
None	11	8
Very little	22	17
A fair amount	55	41
A great deal	40	29
No answer	6	5

Present competence as a psychotherapist (self-estimate):

	N	%
Fully competent	31	23
Reasonably competent	67	50
Slightly competent	17	13
Not very competent	17	13
No answer	2	1

Theoretical orientation:

	N	%
Freud—Orthodox	13	10
Sullivan	27	20
Alexander	8	6
Psychoanalytically oriented—general	75	56
Eclectic	10	7
Meyer	9	7
Miscellaneous	13	10
No answer	15	11
	170*	

* Exceeds number of cases since there were some multiple responses.

Quantitative analysis and statistical treatment

Quantitative analysis of therapists' communications. The systematic analysis of several thousand comments given by over 200 therapists required some method of quantification. A system of analysis was employed whose development and operational characteristics are described in greater detail in another publication (6). The system yields five measures for any therapist communication. There are two sets of categories (*Type of Therapeutic Activity* and *Dynamic Focus*) and three intensity scales (*Depth-directedness*, *Initiative*, and *Therapeutic Climate*). The components are as follows:

Type of Therapeutic Activity specifies the outer form or structure of a therapeutic intervention and provides a gross analysis of the therapist's techniques. The major categories are:

Facilitating communication (minimal activity)
Exploratory operations
Clarification (minimal interpretation)
Interpretive operations
Structuring

Direct guidance
Activity not clearly relevant to the task of therapy
Unclassifiable

Certain subcategories serve to refine the primary ratings.

Depth-directedness is based on the conception that inference is an integral part of all therapeutic communications and that it is always present to some degree. Each communication is rated by means of an eight-point scale:[5]

0	1	2	3	4	5	6	7	8
Non-inferential		Mildly inferential		Moderately inferential			Highly inferential	

Dynamic Focus refers to the frame of reference adopted by the therapist at a particular juncture and characterizes the manner in which he focuses the therapeutic spotlight. Two major sectors are used to differentiate whether the therapist "goes along" with the patient (*A*) or whether he introduces a different focus (*B*). Sector *A* includes silences, passive acceptance, simple reflections of feeling, and minimal questions. Sector *B* is analyzed in terms of five subcategories:

Requests for additional information
Focus on dynamic events in the *past*
Focus on dynamic events in the *present*
Focus on the dynamics of the therapist-patient relationship (analysis of the transference)
Focus on the therapist-patient interaction in terms of the therapist's role as an expert, authority, etc.

Initiative measures the extent to which the therapist assumes responsibility for guiding the patient's communications in a given channel. Seven degrees are distinguished:[6]

0	1	2	3	4	5	6	7
Absence of Initiative		Mild Initiative		Moderate Initiative		Strong Initiative	

Therapeutic Climate or emotional overtones discernible in a communication are quantified by means of a bipolar scale:

−2	−1	0	+1	+2
Coldness Rejection	Milder degrees of −2	Neutrality	Milder degrees of +2	Warmth Acceptance

Rater agreement. To estimate the reliability of the system a random sample of 20 answer sheets was selected and scored independently by two raters.[7] The indices presented in Table 2 are based on a unit-by-unit analysis. Agreement on a unit (therapist communication) means that both raters assigned it to the same category, or that they gave it an intensity score no more than one-half step apart. It was difficult to test rater agreement on *Therapeutic Climate* because of the relative infrequency of nonzero scores. There is reason to believe that scores on this dimension are heavily influenced by voice inflections and other nonverbal clues that are inevitably lost in written materials. Since the evidence in many instances was insufficient so that a "neutral" (zero) score had to be assigned, it may be assumed that the scores on this component almost certainly represent a gross underestimate.

The over-all disagreement was 11.2 per cent, which is considered acceptable. Since most ratings were made jointly by two raters, thus permitting discussions and resolutions of disagreements, the stability of the

TABLE 1

SUMMARY STATISTICS OF AGE, EXPERIENCE, PERSONAL ANALYSIS

	N	Mean	*SD*
Age	131	38.25 years	10.67
Experience in therapy	131	6.78 years	7.22
Personal Analysis	74*	384.19 hours	245.22

* Includes respondents who responded affirmatively and indicated the number of hours.

TABLE 2

AGREEMENT BETWEEN TWO INDEPENDENT RATERS

System Component	Agreement on 20 cases (Number of judgments = 1,673)
Type	86.9%
Depth-Directedness	94.7%
Dynamic Focus	77.9%
Initiative	96.0%
Therapeutic Climate	17 nonzero judgments were made, of which 4 were disagreements.

Note.—All percentages are significant beyond the .01 level

[5] This scale is identical to the four-point scale previously described (6). The expansion of the scale merely represents the introduction of "half-steps" which have been found useful. The original conception remains unchanged.

[6] See Footnote 5. In this case, the original scale consisted of four major scale steps.

[7] Reliability data for typescripts of actual interviews have been reported previously (see 7).

ratings used in the subsequent analyses is probably even higher.

Statistical treatment. The statistical analyses, for the most part, were concerned with the systematic comparison of therapists' response distributions grouped according to one or more independent variables. While all analyses imply at least a crude hypothesis, the fact remains that the work is to a large extent exploratory. For this reason, it appeared important to accept conservative standards; accordingly, the .05 level of probability (two-tailed test) has been used as a minimum level for rejecting the null hypothesis. Since the system components are correlated, the danger of compounding statistical significance when comparing the results on the various components has also been kept in mind. In view of the crudeness of the measurements it seemed prudent to avoid the assumptions underlying such statistical techniques as the t test and analysis of variance and to use instead their nonparametric counterparts (5).

More specifically, comparisons were performed separately on each component of the system. In the case of categories, analyses were based on the distribution of frequencies within a category. For example, a therapist giving 10 "silent" responses was given a score of 10, etc. In the case of the continua, the hypothesis was tested that the more intense scores discriminated between groups of therapists. To maintain the ordinal character of the scale, we compared by the chi-square technique the proportion of therapists who obtained *any* scores of 5 or above on these scales. A similar procedure was followed for scores on *Therapeutic Climate*, where greater importance was assigned to nonzero scores. Negative ("cold," rejecting) scores were judged particularly critical. Consequently, the comparisons involve the proportion of therapists in a given group who gave negative or positive scores respectively, the two kinds of scores being treated independently.

The major statistical techniques were the Mann-Whitney U test (when two independent samples were being compared) and the Kruskal-Wallis one-way analysis of variance (when more than two independent samples were being tested).

Finally, to quantify therapists' questionnaire responses, many of which were of the free-answer type, it was necessary to develop content codes after surveying the range of response for each item. All free-answer questions were then scored jointly by two trained raters.

RESULTS

Diagnostic and prognostic evaluations

Diagnosis. This was the first item in the questionnaire, and required the respondent to indicate the patient's dominant personality type. As shown in Figure 1, eight precoded alternatives, taken from an article by Raines and Rohrer (4), were listed. "Anxiety" was the preferred label, given by almost 40 per cent of the respondents. Actually, this is no more than a phenomenological description since the patient had mentioned the word on numerous occasions. Multiple diagnoses tended to include "Anxiety" in most instances. "Hysteria" and "Paranoid" were chosen with almost equal frequency, followed by "Obsessive," "Character Disorder," "Psychopathic" and some minor ones.

Defense mechanisms. Most prominently mentioned were projective tendencies, including paranoid trends. Rationalization and intellectualization ranked next, followed by somatization, etc. On the average, two or more defense mechanisms were mentioned, the nomenclature itself showing wide variations.

Formulations of dynamics. In this free-response item, respondents were requested to formulate the dynamics of the patient's difficulties. Four major classes were defined, as illustrated by the following examples:

1. Nondynamic formulations

He is a dependent person who felt not adequate to cope with his problems.

Dynamics formulated around family interpersonal relationships.

The patient has never matured emotionally beyond the point of infancy or early childhood.

Hostile dependency. Symptoms probably a reaction to rejected need for dependency.

2. Genetic formulations

a. Descriptive

Seductive dominating mother, weak ineffectual father. Very dependent male with difficulty in identification with either parent.

Unresolved Oedipal conflicts and guilt in death of father.

Patient lost capacity for healthy self-assertiveness or appears to have been incapable of changing direction. He was thoroughly castrated by mother. Dad?

b. Operational

An older indulged son feels displaced by a younger sibling, reacts with anger at being no longer the baby, but is forced to repress his anger.

The patient probably felt rejected by his mother and identified with his father who left him "to hold the bag" so to speak, and brother, identified with mother, was accepted.

His identity as a male is seriously disturbed, marked by fixation at an infantile level, marked castration anxiety, assumption of narcissistic feminine identification with a castrating mother.

3. Emphasis on Current Situation

a. Descriptive

Obsessive-compulsive character traits with self-defeating operations and self-burdening; strong guilt feelings over responsibility toward mother and wife.

Never freed himself from mother. His aggression is tremendous but can't find acceptable expression.

Chronic morbid resentment state based on unsatisfied dependency needs, "the world owes me a living."

b. Operational

Intense hostility toward a domineering mother, who expected a strong sense of responsibility which he must defeat in his neurotic actions.

The patient's anxiety attack started with his being thrown against a "fat woman," which caused a "warm feeling." The following phenomena represent his attempt to defend himself against awareness of longing for dependency on his mother, which he felt momentarily satisfied in contact with the fat woman.

Onset situation incomplete. Rage at mother and wife; impulse to hurt leads to rising titre; resentment at woman hemming him in on streetcar; streetcar almost hits truck equated with his almost out-of-control impulses leading to panic.

4. Formulations combining genetic and current emphases

a. Descriptive

Domineering, manipulative, phobic mother with passive father. Patient feels many unsatisfied dependency needs which he tries to wring from the environment.

A closely bound-to-mother individual who has had difficulty in reaching any degree of maturity and is angry at the deprivation of his dependent needs. Considerable secondary gain, anger repressed and related to his cardiac symptoms; dependent need related to dizziness.

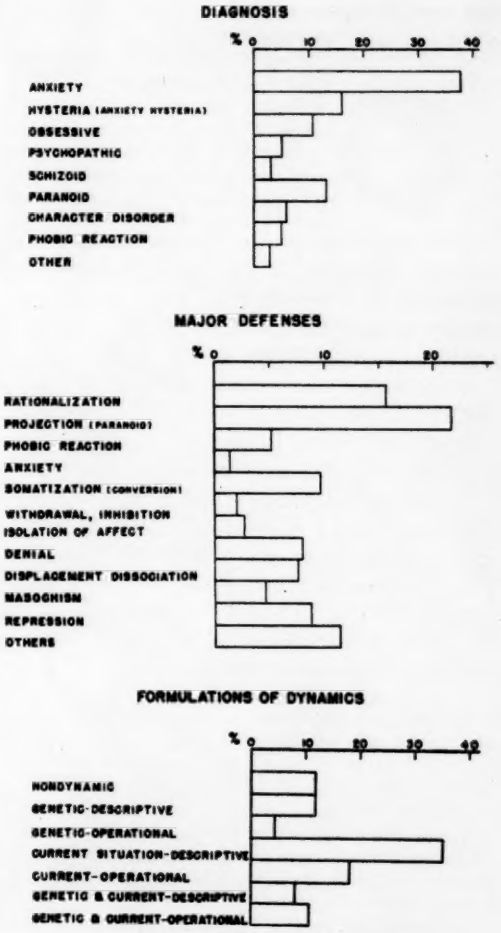

FIG. 1. Diagnoses, Defense Mechanisms, and Formulations of Dynamics.

Passive orientation to father, ambivalence to mother. Easily corruptible superego, with many dependency strivings.

b. Operational

Intense rage at the enveloping mother and the abandoning father, handled by developing anxiety to keep him away from situations in which he could directly express his feelings . . . underlying love for mother beneath the rage.

Guilt and anxiety leading to incorporative attitudes. Regressive oral demanding attitudes toward mother repressed because of fear. Illness of his wife followed by mother's leaving home left patient without fulfilment of dependent needs. This resulted in insecurity and phobic mechanisms to get support. (Symbolic expression of fragmentary incorporative attitude toward mother in G. I. symptoms.)

Hostile identification with mother is basic is-

sue; underlying depression in relation to father's death with suppressed rage and acting out as alternative defenses against passive (homosexual) trends. Suspect patient is hypertensive.

Most likely dynamics is rage at father (seen as a maternal but absent person), defended by phobic displacement, obsessive-compulsive mechanisms, etc. These are not sufficient to ban anxiety, and he feels anxiety directly.

The major categories are arranged approximately in order of increasing complexity, although no prejudgment was made about the relative positions of genetic and current formulations. Within the categories, an "operational" statement probably makes greater demands on the respondent's ability to make inferences than a "descriptive" one, which remains closer to observable events.

It seemed desirable, therefore, to judge formulations by an over-all criterion which came to be known as "Dynamic Quality." This rating was made on a four-point scale, ranging from 1 (low) to 4 (high). In arriving at this global judgment, the following criteria were kept in mind:

a. Complexity. Does the formulation attempt to explain one or several facets of the patient's problem? Does it integrate a number of seemingly disparate manifestations? Is it a "rich" or a "meager" formulation?

b. Degree of inference. Is the formulation inferential or does it remain at a descriptive level? Does it state or imply a hypothesis which may have a bearing on therapy?

c. Precision. Is the language precise or is it "loose"? Are technical terms used in a strict sense or is technical language used as a jargon? Does a formulation seem to apply to the patient specifically or is it worded so generally that it might apply to anyone?

d. Operational aspects. Does the formulation indicate how the patient "handles" his conflict or does it describe a state? Is the conflict set forth in terms which are at least potentially verifiable or is the likelihood of verification virtually nonexistent?

Rater agreement on the over-all rating, between two independent raters, was 88 per cent (disregarding discrepancies of one scale step).

Other diagnostic evaluations. Several diagnostic assessments were obtained through precoded items. Each item was followed by a four-point intensity scale, and the added alternative "Too little evidence to make a judgment at this time." Response distributions are presented in Figure 2. (In some cases, categories containing very small frequencies were combined with the adjoining one.) The alternative "Too little evidence" actually lies outside the scale continuum and signifies a respondent's refusal to make a judgment. It may also indicate the degree of inference involved in a rating, as perceived by the respondents. Thus, Ego Strength shows 16.5 per cent of the responses in that category, whereas Disturbance discloses less than 1 per cent. By this standard, therapists by and large thought there was sufficient evidence on such items as Anxiety, Insight, Self-Observation, Emotional Maturity, and Disturbance but they expressed greater tentativeness on Ego Strength, and Social Adjustment. Relatively few respondents refused to commit themselves.

The distributions disclose fair agreement among the respondents on the relative intensity of the variable being rated. The quantifications are, of course, crude, and part of the agreement is accounted for by the small number of available scale steps. For example, the subjective difference between "a great deal" of anxiety and "a fair amount" of anxiety may be slight, but the discrepancy between "a great deal" and "relatively little" anxiety seems considerable. In this case, 85 per cent of the respondents agreed that the patient experienced at least a fair amount of anxiety, but 11 per cent judged it to be "relatively little." The distributions on the other items are comparable, except for Emotional Maturity and Social Adjustment where the divergent minorities of about 10 to 15 per cent observed on other items dwindled to about 1 per cent.

Prognosis. Diagnostic evaluations were followed by two questions relating to prognosis. Respondents were asked: "Assuming your recommendations for treatment were followed, how would you rate the prognosis for this patient?" Responses were made on five-point scales, to which the alternative "Too little evidence" was appended.

Figure 3 shows that more than half of the respondents thought that the patient would remain "about the same" or even

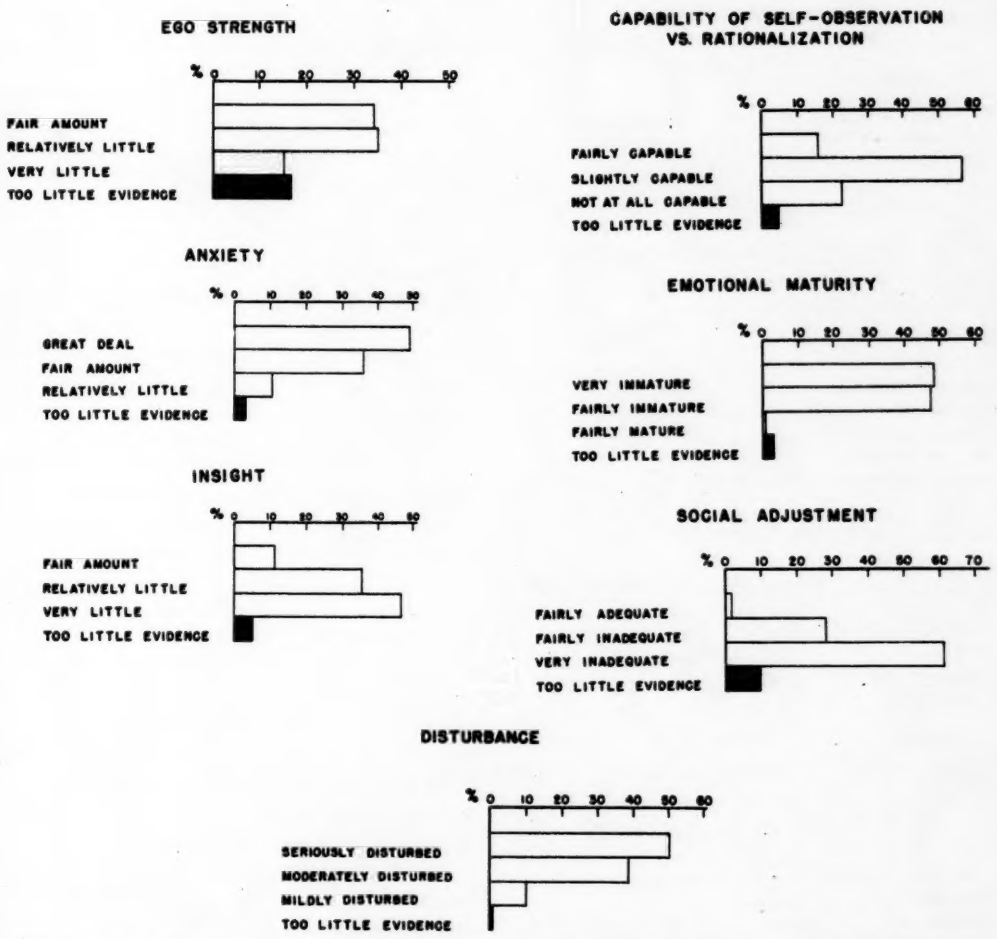

FIG. 2. Ego Strength, Anxiety, Insight, Capability of Self-Observation vs. Tendency for Rationalization, Emotional Maturity, Social Adjustment, and Disturbance.

"get a little better" without therapy, but 40 per cent thought he would "get a little worse" or "much worse." With therapy, the prognosis was seen as favorable by about a third and unfavorable by 45 per cent. These ratings involve relatively complex predictions so that the absence of a consensus is not startling. Also, favorableness of the prognosis partly depends on the respondent's frame of reference, treatment goals, etc. Even if the crudeness of the judgment is conceded, the fact remains that a notable percentage of the respondents took an optimistic view whereas a somewhat more sizable proportion regarded the outlook pessimistically.

Attitude toward the patient. Figure 3 indicates that the proportions of positive and negative attitudes were almost equal; about a fifth professed a neutral attitude, and the reaction of a tenth was ambivalent. When asked for the reasons for their reaction, 43 respondents left the question blank. Of those who replied, 36 per cent referred in one form or another to the patient's "unpleasant personality characteristics"; an almost equal percentage stressed his assets, often with some expression of empathy. It is worth noting that 11 per cent disliked him on what might be called "technical grounds," i.e., poor risk or poor motivation for therapy.

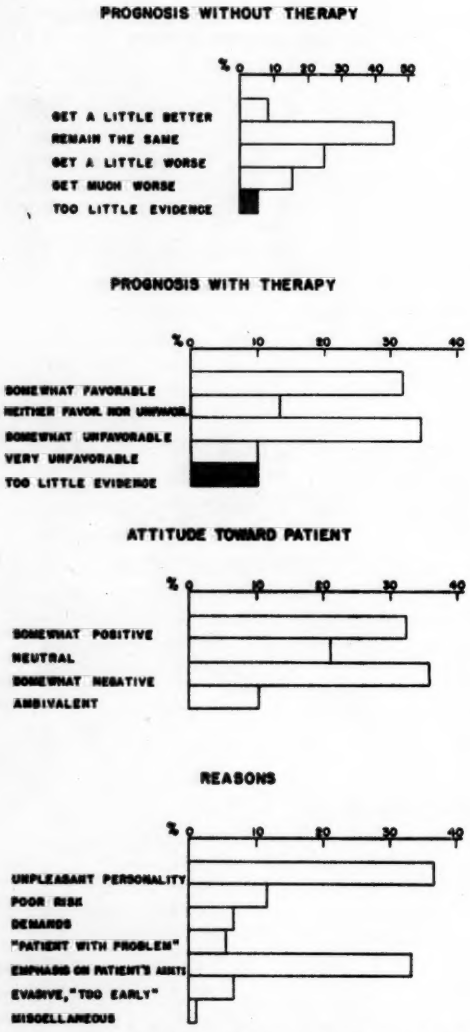

FIG. 3. Prognosis without Therapy, Prognosis with Therapy, Attitude toward the Patient, and Reasons for the Attitude.

Formulations of therapeutic plans, goals, and techniques

Therapeutic goals and approaches (Figure 4). More than half the respondents set rather modest goals consisting of some form of symptom relief. This was true particularly when a distinction was made between intermediate and ultimate goals. Even those therapists who described their goal as "a certain amount of insight" generally regarded this as the ultimate objective. Virtually no therapist indicated that he would strive for extensive personality reorganization. Only three therapists refused to accept the patient for treatment.

If we consider areas in which therapists expected the patient to "move" and areas in which they expected him to remain relatively unchanged, practically no one expected any basic personality change, whereas the possibility of more superficial changes was conceded by a fair proportion. However, a slightly larger proportion did not foresee even this as a likely occurrence. The greatest improvements, relatively speaking, were expected with regard to symptoms, work adjustment, and interpersonal relations, but minorities specified that little or no movement was to be anticipated in these areas. Many therapists took a rather pessimistic view of the patient's disturbance and of his capacity to profit from psychotherapy.

Asked to specify areas or problems which might be most amenable to therapy, therapists mentioned three major areas: (*a*) interpersonal relations (mostly with mother and wife, to a lesser extent with father, brother, and authority figures), (*b*) specific emotional dynamics (self-destructive tendencies, etc.), (*c*) external reality (work, jobs, living arrangements, etc.).

A similar question, designed to elicit information on areas in the patient's living which might receive primary focus, produced a shift in the response pattern: interpersonal relations and external reality were named more frequently and emotional dynamics less frequently. The differences may be a function of the wording of the two questions.

Closely related to the questions of focus were two items pertaining to attitudes or kinds of behavior which the therapist would encourage (or discourage) in therapy with this patient. Encouraged were: (*a*) a sense of responsibility, feelings of worthwhileness, self-esteem, independence, and the patient's masculine role; (*b*) increased socialization, gainful employment, "living in the present"; (*c*) the expression of feelings without guilt; (*d*) the development of insight. Discouraged were: (*a*) intellectualization, obsessive ruminations (resistances); (*b*) self-pity, self-

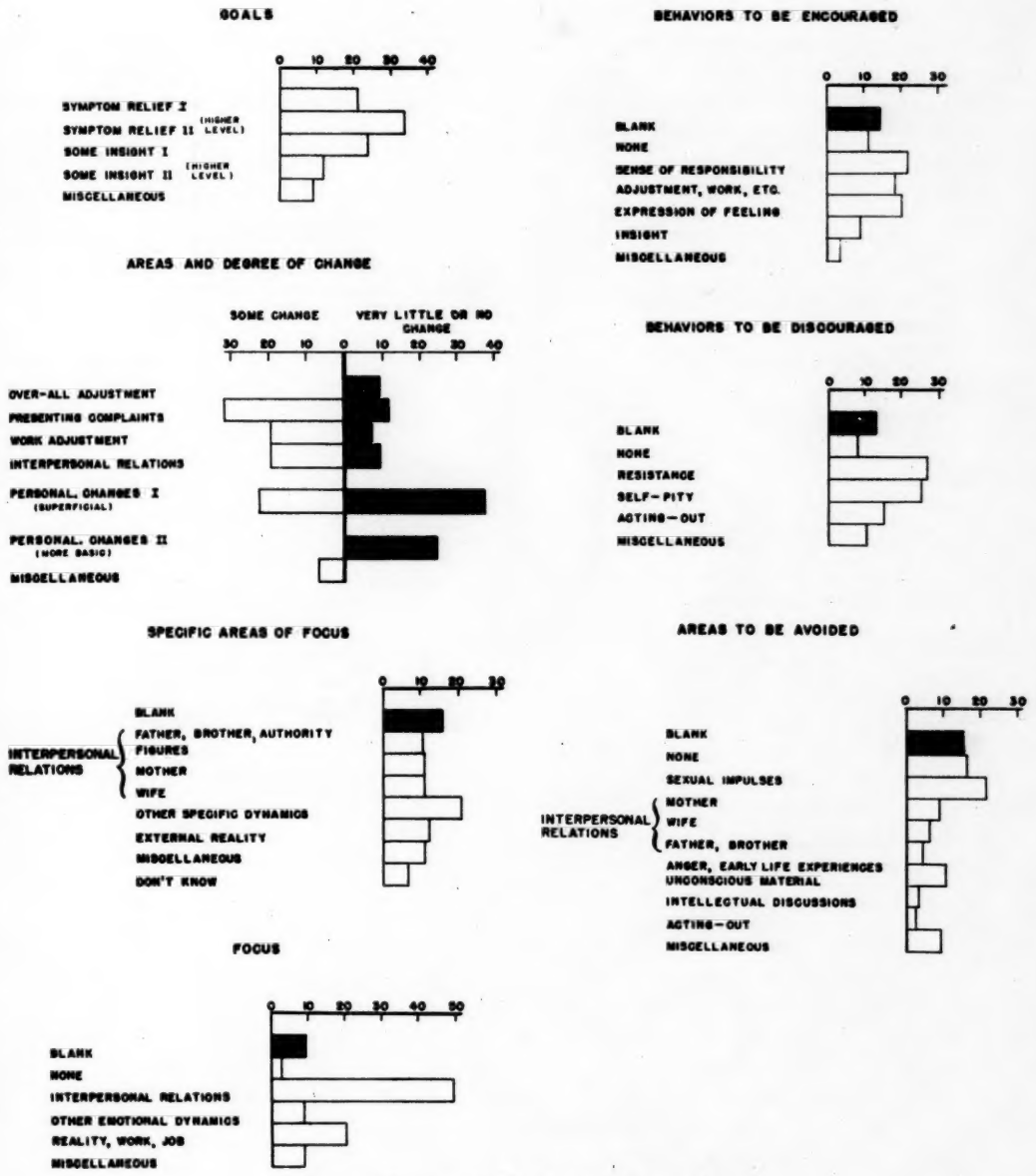

FIG. 4. Therapeutic Goals and Approaches.

depreciation, helplessness, demands; (*c*) acting out of anti-social impulses.

Among areas to be avoided (early) in therapy, the following were mentioned: sexual and homosexual impulses, Oedipal material, and the patient's concept of his own masculinity. Next in frequency ranged interpersonal relations with his mother and wife. Small frequencies were accumulated under the headings of intellectual discussions, anger, hostility, death wishes, early life experiences, unconscious material, fantasies, dreams. Twenty-six respondents specifically indicated, however, that no areas should be avoided. Some therapists counseled the avoidance of certain areas (e.g., interpersonal relations with mother and wife) which were singled out for therapeutic

focus by others. It is conceivable that the time stipulation ("early in treatment") may have been the cause for these apparent contradictions.

Problems of therapeutic technique (Figure 5). In order to explore the interrelationships between perceptions, evaluations, and treatment plans, a question was included

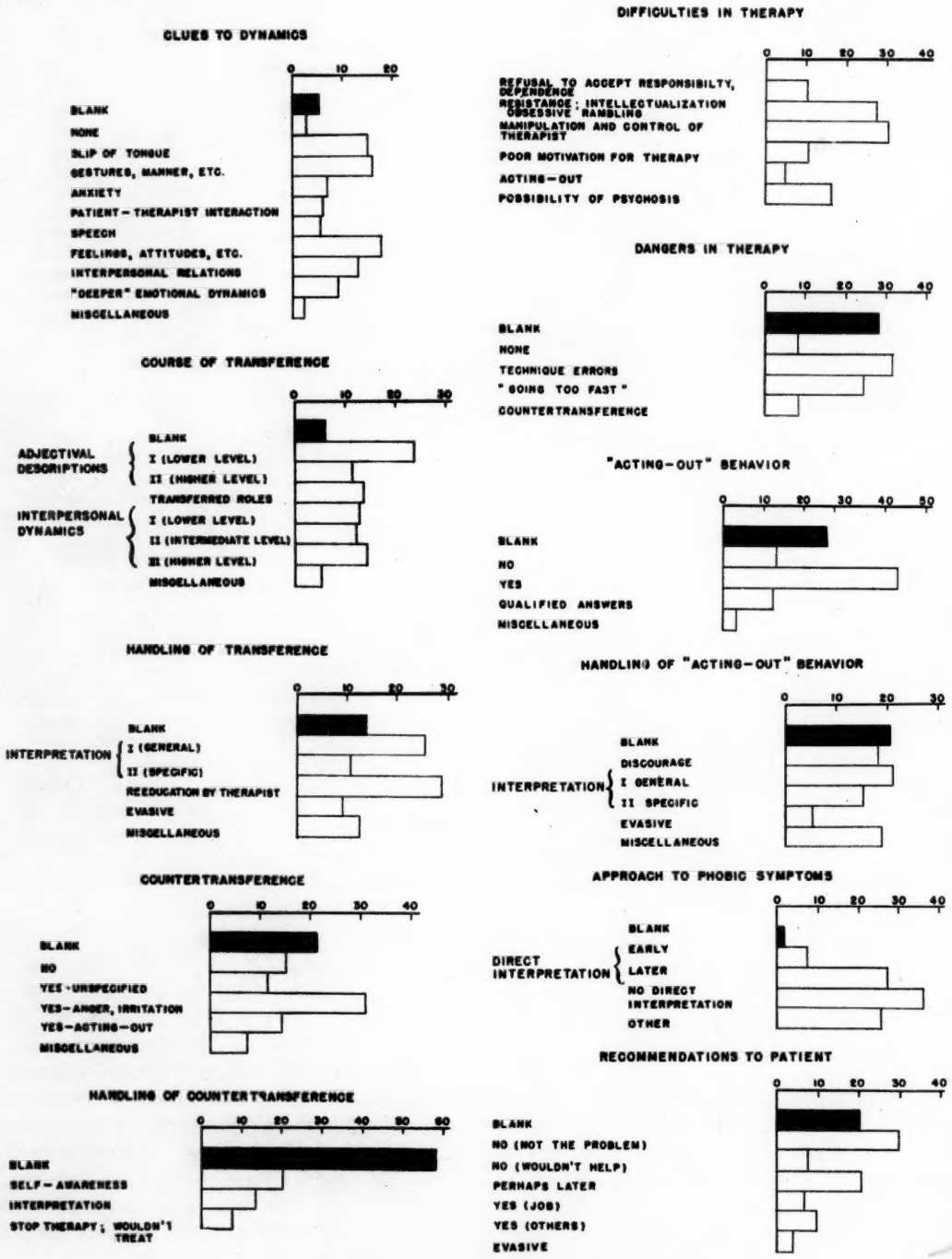

FIG. 5. Problems of Therapeutic Technique.

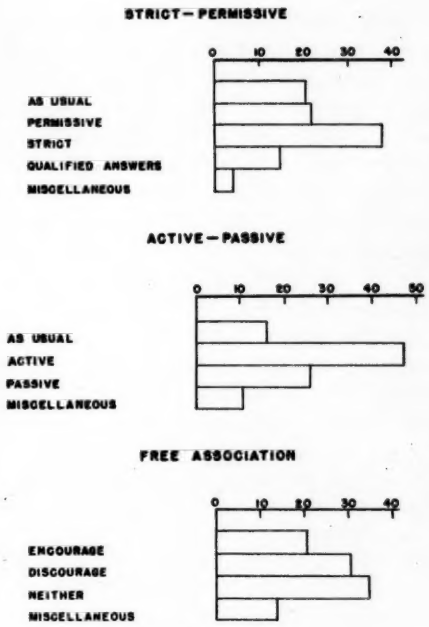

FIG. 5. (cont'd.).

concerning clues in the patient's behavior which the therapist might not wish to use in an initial interview but which he might keep in mind for later reference. Such clues might facilitate inferences concerning the nature of the patient's emotional conflict and generate hypotheses to be tested in subsequent therapeutic sessions.

The majority of the 200 clues referred to observations about the patient's feelings and attitudes (anger, hostility, "the world owes me living") with frequent elaboration of their dynamic significance. Another cluster dealt with nonverbal clues (gestures, manner, bodily movements, etc.). A relatively large number referred to a slip of the tongue occurring in the interview (speaking about his father's death, the patient substituted the word "mother"). References to "deeper dynamics," i.e., highly inferential statements, were comparatively infrequent.

Responses relating to the course of the transference seemed to fall into three major categories: (*a*) adjectival descriptions, (*b*) statements in terms of transferred roles (often tautological), and (*c*) statements in terms of interpersonal dynamics. This sequence implies a hierarchy of increasing complexity and specificity, and it will be shown that the kind of formulation chosen was actually associated with the respondent's length of experience in therapy. The following examples illustrate responses assigned to the major scoring categories:

(*a*) *Adjectival Descriptions*

Negative.
Resentful, complaining.
First positive, then negative.

(*b*) *Transferred Roles*

[Course of the transference is that of] the relationship with his mother.
With me, I would expect him to be a child with father—wanting help with wife and mother.
[He would see me as] Mother, with expectations of being smothered by therapist, too.

(*c*) *Interpersonal Dynamics*

[Transference] Probably would be fairly quickly established and therapist would probably first be the good father. Later, his [patient's] need for dependency gratification would make things stormy.
Possibly anxiety about possible illnesses in therapist. Hostile defensiveness if pressed.
I would expect him to develop hostile feelings which he would not be able to express but which would cause him to have symptoms and perhaps to stay away.

Two principal methods for handling transference problems were suggested: one is best described as interpretive, i.e., pointing out the distortive elements in the patient's maneuvers; the other refers to a skillful use of the therapist's role in changing the patient's attitudes and actions, by firmness, reassurance, or other re-educative techniques. In the latter method, interpretations are not necessarily excluded, but the major emphasis is upon the therapist as a person, authority figure, or reality model.

A patient who is very demanding, aggressive, and manipulative is likely to evoke emotional reactions from the therapist. More than half of the respondents expected to get involved in countertransference reactions with this patient. Most frequently mentioned were anger, annoyance, irritation, and impatience. Less often mentioned was the danger of acting out in response to the patient's manipulative and controlling tendencies, which might lead the therapist to abdicate his therapeutic role. More than 20 per cent of the therapists failed to answer this question. Almost three times as many

therapists did not answer the second part of the question asking for specific statements about the "handling" of possible countertransference reactions. Those who replied stated they would try to be aware of the problem and act accordingly. A smaller number said they would discuss it with the patient or interpret it.

Related to questions about transference and countertransference were several items concerning difficulties and/or dangers that might arise in the therapy. With regard to difficulties, respondents mentioned prominently the patient's attempt to manipulate and control the therapist. Of equal magnitude was resistance, intellectualization, and obsessive rambling. Akin to the foregoing were comments about the patient's refusal to accept responsibility, his dependence and parasitic trends, his projections of blame, and his poor motivation for therapy. The possibility of a psychotic episode was also anticipated by a fair number of therapists.

The question about dangers elicited responses which tended to be centered around techniques, with a variety of problems that could not easily be subsumed under common denominators. Frequent comments referred to an incorrect handling of the transference relationship, and an appreciable number cautioned against the therapist's "going too fast." They suggested the need for a sympathetic and understanding attitude to prevent the patient from becoming too anxious or developing a psychosis.

Three items dealt with more specific technique problems. The first asked whether the respondent expected the patient to engage in much acting out behavior, and, if so, how he would deal with it. The preferred methods for handling the problem were to be interpretation and control by firmness, strictness, or setting limits. Miscellaneous recommendations were made for supportive measures, including reassurance, focus on the patient's personality assets, and re-education.

Considerable divergence of opinion was expressed regarding the optimal method for dealing with the patient's phobic symptoms. The majority considered direct interpretation of these symptoms contraindicated at any time; a somewhat less sizable group thought interpretation should occur at a much later time; another group advocated more or less idiosyncratic approaches; and a small minority thought that the therapist should interpret them as early as possible.

A similarly divergent distribution was obtained on the question of recommendations to the patient to change his present mode of living or his environment. About one-third of the responses were in the negative, with most members of this group pointing out that recommendations were outside the therapist's province and unrelated to the therapeutic problem. Twenty per cent of the respondents qualified their replies, counseling deferment of a decision. Certain respondents indicated a willingness to make recommendations, primarily with respect to the patient's job, living arrangements for his mother, wife, etc.

Another set of questions sought to explore further the therapist's approach to therapy, and to determine the extent to which the therapist's "usual" techniques might be modified by or adapted to the particular set of problems presented by this patient. One out of five therapists indicated that they would be no more strict or permissive with this patient than with any other. A like number preferred permissiveness. Forty per cent, however, voted for strictness, basing their judgment mostly on technical requirements. A minority qualified their responses by stating they would be strict in some areas, permissive in others, etc.

A question about the therapist's level of activity was worded as follows: "Assuming that the patient's behavior during the first hour is rather typical of him, would you tend to be more active (i.e., intervene frequently) or more passive (i.e., intervene infrequently) in subsequent interviews?" Here the division of opinion appeared even more pronounced than in the preceding item: Almost 50 per cent said they would tend to be rather active; a quarter said "rather passive"; others qualified their response or gave a noncommittal answer.

Free association was discouraged by a third; encouraged by a fifth; neither encouraged or discouraged by another third.

Form of therapy, frequency of sessions,

and length (Figure 6). The preferred method of treatment was described simply as "psychotherapy." Thirty respondents qualified their responses by adding such terms as "intensive," "insight," "uncovering," or "analytically oriented." It is not certain, however, that those respondents who failed to define "psychotherapy" more closely did not in fact refer to more intensive therapy. Psychoanalysis, as the most extensive and intensive form of therapy, was recommended by less than 10 per cent. Other minorities recommended supportive therapy, group therapy, and various combinations.

Further information on the method of treatment—at least on its external aspects—is provided by the frequency of weekly sessions suggested by the respondents. The modal response was 1–2 times per week. Almost equally preferred, however, were interviews at the rate of 2–3 times per week. A sizable minority thought the patient should be seen only once a week. Therapists who voted for three or more sessions per week comprised only 10 per cent.

Considering the divergence of goals and modes of treatment, it is not surprising that the length of therapy was estimated quite differently. Therapists who distinguished between intermediate and ultimate goals expected to achieve the former in one year or less whereas the latter were estimated most prominently in the 2–3 year range. As many therapists anticipated the treatment to last 6 months or less, as 3 years or more. An appreciable proportion gave evasive answers.

A more specific question dealt with the length of time required to relieve the patient's symptoms. The majority estimated that this could be done in one year, a fair percentage being even more optimistic. To the extent that the therapist views symptoms as rooted in the patient's personality structure whose modification is the more important task of psychotherapy, he might feel reluctant to take a stand on a question

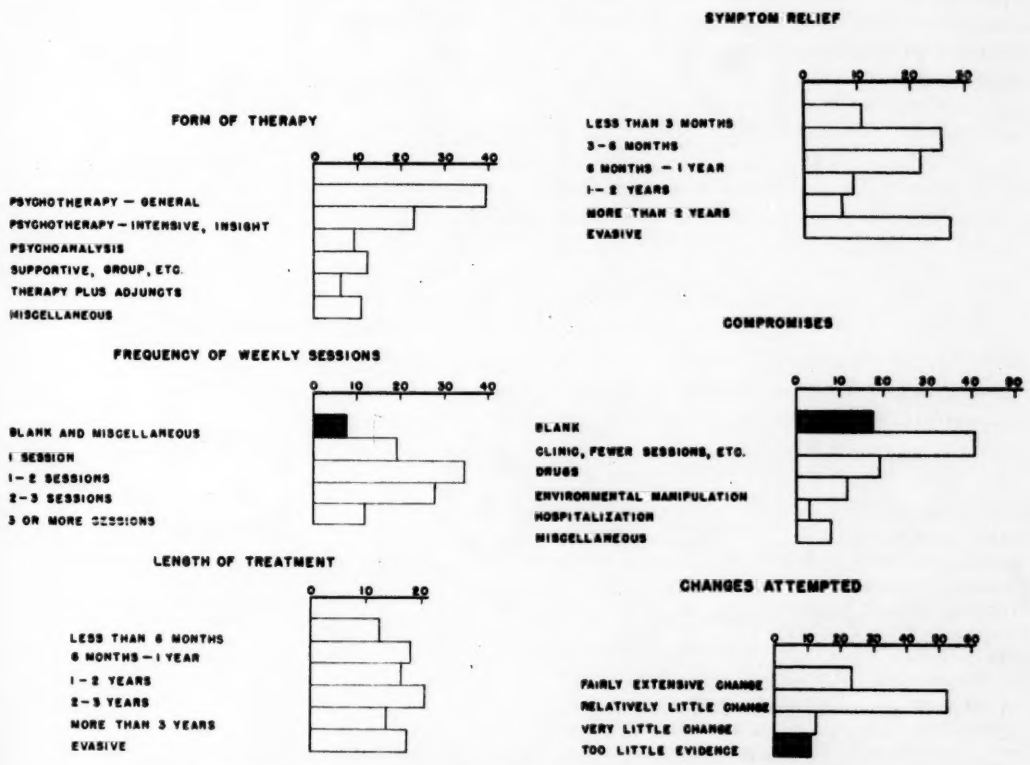

FIG. 6. Form of Therapy, Frequency of Sessions, and Length of Therapy.

which might strike him as thoroughly nondynamic. The large percentage of abstentions or noncommittal answers on this item may be partly explained by this reasoning.

In view of the patient's poor economic condition, his wife's invalidism, etc., it appeared appropriate to consider the possibility of compromises in case the recommendations for treatment were unfeasible for practical reasons. The most frequent response concerned changes in external arrangements of therapy, such as fewer sessions per week, referral to a clinic, or less intensive treatment. A moderate proportion suggested such adjuncts as tranquilizing drugs. It is noteworthy that negligible minorities recommended hospitalization or electric shock therapy.

As a global estimate of the kinds of changes in the patient's personality structure the respondents would attempt, the following responses appear instructive: A quarter of the responding therapists described their over-all goals as a fairly extensive change; more than half contented themselves with relatively little change; and 12 per cent said they would strive for very little change.

Analysis of systematic differences

Variability as a function of experience. In view of the wide range of professional experience among the therapists analyses were performed to determine the degree of association between length of experience and the various questionnaire items. The sample was divided into three groups: Group I included psychiatrists having 0 to 1¾ years of experience in therapy; Group II, 2 to 5 years of experience; and Group III, 6 years and above. Responses to each questionnaire item were then broken down in terms of this grouping, and the chi-square technique applied to test the significance of the differences in the three groups.

The results can be summarized by the statement that length of professional experience seemed to have a negligible bearing upon therapists' diagnostic and prognostic evaluations. In the case of continuous variables, product-moment coefficients of correlation were also computed. These are presented in Table 3.

The significant correlation between Ego Strength and experience as well as age indicates that with increasing experience thera-

TABLE 3

PRODUCT-MOMENT COEFFICIENTS OF CORRELATION BETWEEN ATTITUDINAL AND BIOGRAPHICAL VARIABLES

(Average N = 115)

Variable	Prognosis with Therapy	Attitude toward Patient	Experience (in years)	Experience (self-estimate)	Competence as Therapist (self-estimate)	Age
Ego Strength	.37**	.15	.20*	.07	.03	.23**
Anxiety	.08	.20*	−.09	−.03	.00	−.12
Insight	.38**	.33**	.12	.11	−.05	.03
Self-Observation vs. Rationalization	.36**	.35**	−.10	−.02	.04	−.09
Emotional Maturity	−.41**	−.20*	.05	.04	.03	.03
Social Adjustment	.28**	.21*	.15	.08	−.09	.10
Disturbance	−.17	−.04	−.07	−.10	.02	−.05
Prognosis without Therapy	.05	−.11	.14	−.01	.08	.21*
Prognosis with Therapy	—	.39**	.16	.07	−.08	.06
Attitude toward Patient	—	—	.02	.05	−.09	−.11
Experience (in years)	—	—	—	.72**	−.65**	.85**
Experience (self-estimate)	—	—	—	—	−.80**	.64**
Competence as Therapist (self-estimate)	—	—	—	—	—	−.56**

* Significant at the .05 level.

** Significant at the .01 level.

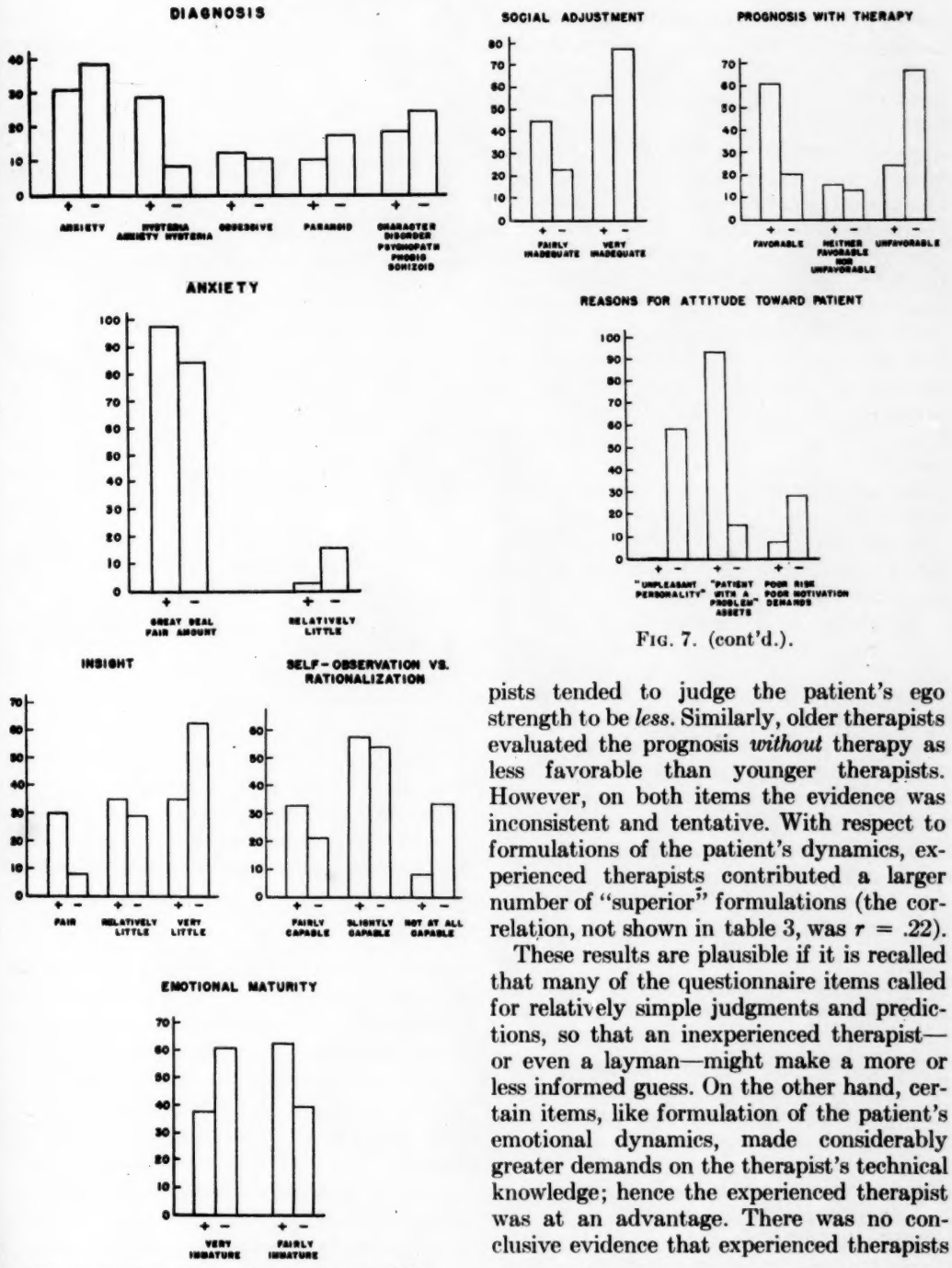

FIG. 7. Clinical Evaluations in Terms of Therapist's Attitude Toward the Patient, Based upon Statistically Significant Chi Squares. Each bar indicates the percentage of therapists choosing a particular response alternative depending upon their attitude toward the patient. (+ = positive attitude, $N = 40$; − = negative or ambivalent attitude, $N = 65$; neutral attitude, $N = 28$, omitted.)

FIG. 7. (cont'd.).

pists tended to judge the patient's ego strength to be *less*. Similarly, older therapists evaluated the prognosis *without* therapy as less favorable than younger therapists. However, on both items the evidence was inconsistent and tentative. With respect to formulations of the patient's dynamics, experienced therapists contributed a larger number of "superior" formulations (the correlation, not shown in table 3, was $r = .22$).

These results are plausible if it is recalled that many of the questionnaire items called for relatively simple judgments and predictions, so that an inexperienced therapist—or even a layman—might make a more or less informed guess. On the other hand, certain items, like formulation of the patient's emotional dynamics, made considerably greater demands on the therapist's technical knowledge; hence the experienced therapist was at an advantage. There was no conclusive evidence that experienced therapists

preferred a particular kind of formulation, as measured by the classifications mentioned.

These tests also proved nonsignificant almost without exception on questions dealing with therapeutic plans, goals, and techniques. A parallel analysis comparing analysts (mean experience 13.7 years), psychiatrists (mean experience 7.3 years), and residents (mean experience .9 years) was likewise inconclusive except that analysts tended to judge the length of therapy to be longer than either psychiatrists or residents (who did not seem to differ significantly) and analysts preferred 3 or more weekly sessions, in contrast to psychiatrists who preferred 1 or 2 weekly interviews.

In both analyses, the more experienced therapists, irrespective of whether they described themselves as analysts, tended to state the course of the transference in terms of interpersonal dynamics, whereas inexperienced respondents tended to prefer descriptions in terms of transferred roles.

Variability as a function of attitude toward the patient. The next analysis concerned a breakdown of responses to each questionnaire item in terms of the therapist's attitude toward the patient.

Table 3 shows a positive correlation between prognosis and attitude. It is not surprising that prognosis is correlated with diagnostic evaluations, but it is noteworthy that most of the relationships shown in Table 3 are paralleled by the therapist's attitude toward the patient. A slightly different method of analysis lends greater concreteness to these findings (see Figure 7). In each instance, chi squares were computed for the distributions of the positive and negative groups; the small ambivalent group was included in the negative group; the neutral group, because of its relative smallness, was omitted. The results were significant at least at the .05 level, and usually at the .01 level or beyond.

Therapists indicating a negative attitude toward the patient were more likely to choose such diagnostic labels as psychopath, character disorder, paranoid, and phobic; tended to see the patient as less anxious, having less insight, being less capable of self-observation, more immature emotionally, and more poorly adjusted socially. A negative attitude toward the patient was associated with significantly poorer prognosis, and therapists responding in this manner were more likely to comment about the patient's "unpleasant personality characteristics."

TABLE 4

COMPARATIVE DATA ON PROBABILITY LEVELS OF CHI SQUARE FOR FULL SAMPLE AND PSYCHIATRIST SAMPLE

Topical Area and Questionnaire Items	Full Sample ($N=168$)*	Psychiatrists only ($N=105$)**
Goals and Approaches		
Areas to be Avoided	.01	NS
Problems of Therapeutic Technique		
Dangers in Therapy	.02	NS
Handling of Countertransference	.05	NS
Recommendations to Patient	.001	.02
Strict-Permissive	.001	.05
Active-Passive	.05	.01
Free Association	.01	NS
Form of Therapy, Frequency, Length		
Form of Therapy	.05	.05
Frequency of Sessions	.05	.05
Symptom Relief (time estimate)	.001	.05
Changes Attempted	.05	NS

*"Neutral" group omitted; "ambivalent" group omitted.
**"Neutral" group omitted.
NS = Not significant.

Of the 27 chi squares relevant to therapeutic goals and techniques, 6 were significant at or beyond the 5 per cent level. It will be seen from Table 4 that the number of significant chi squares is almost doubled when the statistic is computed on the full sample of therapists (psychiatrists, psychologists, and social workers). Since the direction of change is quite consistent, it may be assumed that the trends are attenuated when the size of the sample is reduced. It appears less likely that the observed trends are more pronounced in the nonpsychiatrist groups.[8]

[8] The experience levels of the various attitudinal groups, on the other hand, are more sharply differentiated for psychiatrists than for the full sample. In the former group, therapists professing a positive attitude had a mean experi-

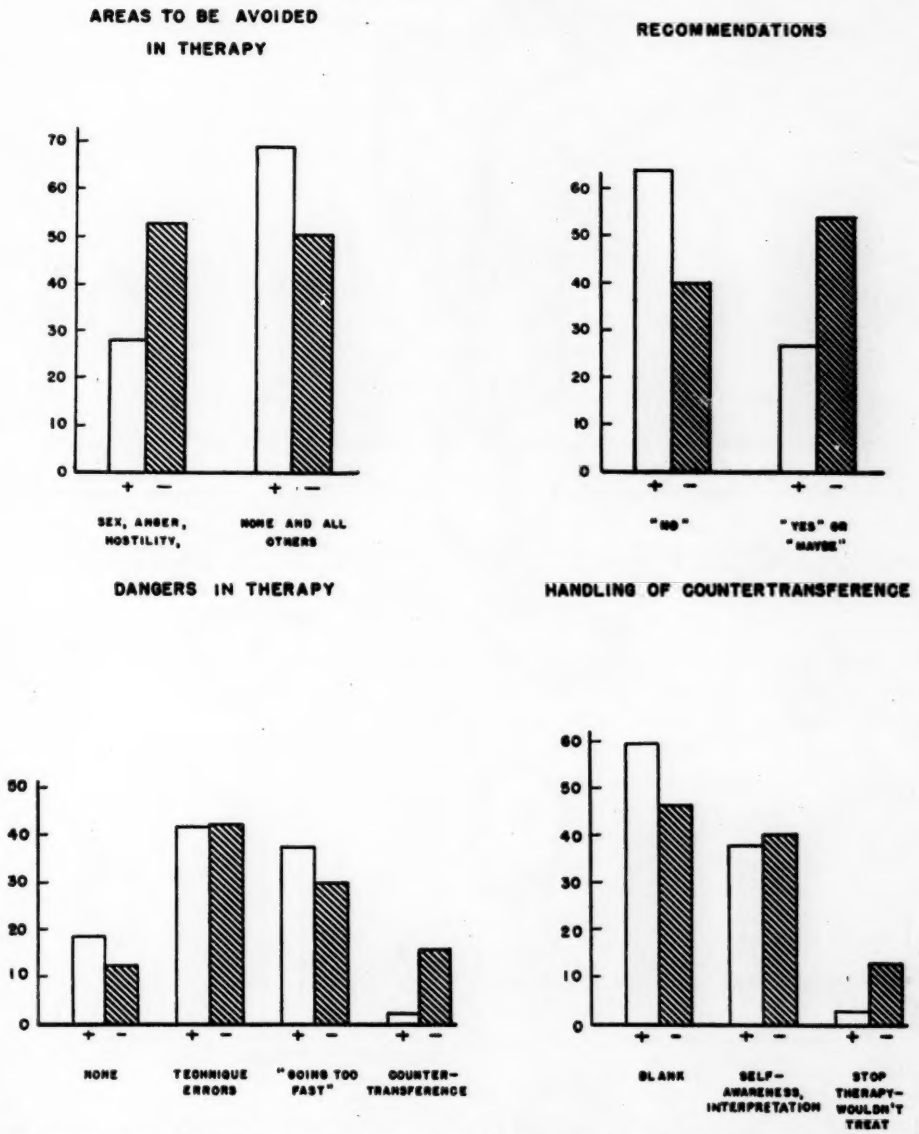

FIG. 8. Responses in Terms of Therapist's Attitude Toward the Patient, Based upon Statistically Significant Chi squares. Each bar indicates the percentage of therapists (full sample) choosing a particular response alternative depending upon their attitude toward the patient. (+ = positive attitude, $N = 75$; − = negative attitude, $N = 93$; neutral attitude, $N = 45$, and ambivalent attitude, $N = 21$, omitted.)

While greater confidence, obviously, can be placed in those results which are significant in both samples, it seemed justifiable to present graphically the results for the full sample. This has been done in Figure 8.

Only one of the questions pertaining to therapeutic goals and approaches yielded a significant chi square, and this may be due to the particular grouping of responses. This result indicates (full sample only) that therapists whose attitude toward the patient

ence of 4.72 years; neutral, 8.46 years; negative and ambivalent, 6.55 years. Analysis of variance yielded an F of 15.74, which is significant at the .001 level. For the full sample, the mean differences were statistically nonsignificant.

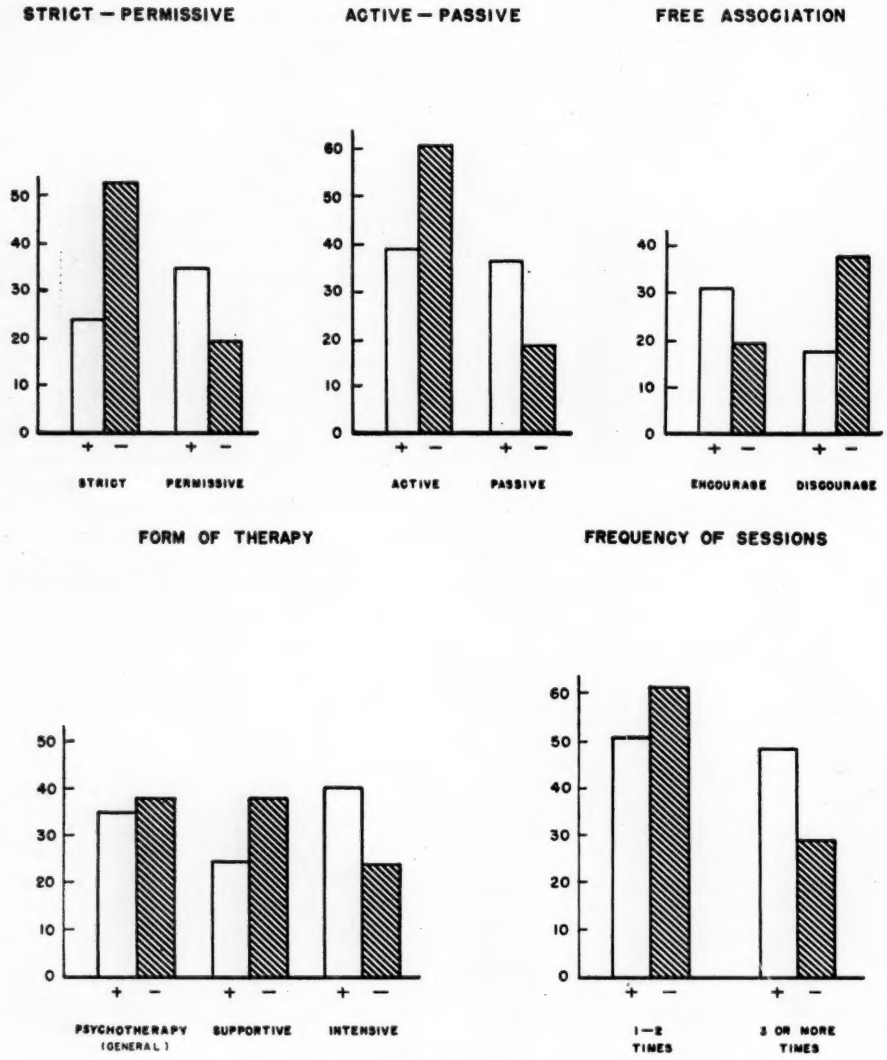

FIG. 8. (cont'd.).

was negative were more likely to suggest avoidance of certain areas in therapy, such as: the patient's relationship with his wife, sexual impulses, hostility, anger, death wishes, unconscious material, fantasies, and dreams.

Under the heading "Problems of Therapeutic Technique," the null hypothesis was rejected for several questionnaire items. Descriptively, these results may be stated as follows:

Therapists expressing a negative attitude toward the patient were more likely to anticipate dangers to successful therapy from the therapist's anger, irritation, and other countertransference reactions (full sample only). On the other hand, therapists holding a positive attitude were more likely to mention the danger of pressing the patient too hard in therapy and recommended a sympathetic and understanding attitude by the therapist. While there was a slight (but inconclusive) trend for the "dislike" group to expect more countertransference reactions, this group was more likely to discontinue therapy if the therapist became aware of countertransference, or not to accept the patient for treatment in the first place. Therapists admitting a negative attitude were more likely to make recommendations to the patient concerning changes in his mode of living—an

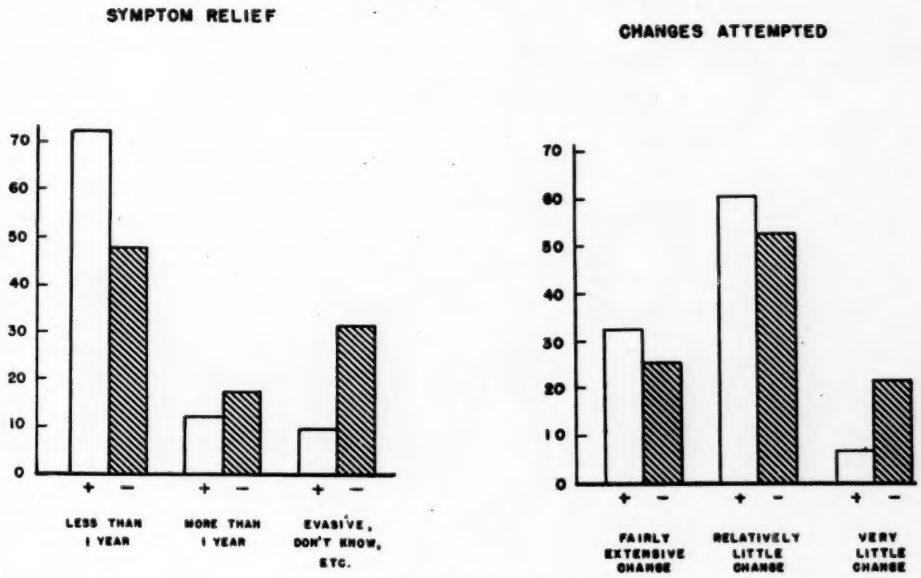

FIG. 8. (cont'd.).

approach often strongly disadvised by the "like" group.

The evidence was clear-cut that therapists whose attitude toward the patient was positive tended to characterize their proposed techniques as permissive, whereas a significantly larger proportion of the "dislike" group voted for strictness.

Similarly, with respect to the therapist's activity or passivity, a positive attitude toward the patient was associated with passivity, a negative attitude with activity.

Free association tended to be encouraged by therapists feeling positively about the patient, but was discouraged by those holding a negative attitude (full sample only).

With regard to the form of therapy, frequency of sessions, etc., a positive attitude was associated with the more intensive forms of treatment, including psychoanalysis, whereas therapists feeling negatively toward the patient were more likely to recommend supportive therapy or other adjuncts. A positive attitude was correlated with more frequent weekly visits, fewer evasive answers about symptom relief, and plans for more extensive change in the patient's character structure (full sample only). Responses to the last item indicated that compared to therapists expressing a positive attitude, four times as many therapists holding a negative attitude planned for "very little change."

Attempts were also made to explore the possible influences of *theoretical orientation* and *personal analysis* upon therapists' response patterns. With respect to theoretical orientation, it proved difficult to isolate "pure" groups since respondents often acknowledged multiple influences. Several criteria were used to define "orthodox" and "neo-Freudian" groups, but these were so small as to preclude the application of rigorous statistical techniques. The results of this analysis must therefore be regarded as inconclusive. Similar difficulties were encountered with regard to personal analysis. Here an attempt was made to match a sample of psychiatrists whose training had included personal analysis with a comparable nonanalyzed group, experience, age, and sex being held constant. Even with an N of 28 in each group it proved impossible in most instances to test for the significance of differences on the various item responses. While the evidence must be judged indeterminate, it may be worth mentioning that the observed differences appeared, for the most part, small.

Analysis of therapists' communications to the patient

Over-all response distributions. Table 5 presents frequencies and percentages of therapists' responses, as distributed within each system component. As might be expected in a first interview, many communications were designed to obtain a fuller

TABLE 5

SUMMARY DISTRIBUTIONS OF PSYCHIATRISTS' RESPONSES, QUANTIFIED BY A MULTIDIMENSIONAL SYSTEM OF ANALYSIS

(N = 126 Psychiatrists)

System Component	Responses N	Per Cent
Type		
Silence	975	27.3
Passive Acceptance	169	4.7
Exploratory Operations	1,689	47.3
Clarification	128	3.6
Interpretive Operations	175	4.9
Direct Guidance	302	8.5
Miscellaneous and Unclassifiable	134	3.7
Total	3,572	100.0
Depth-Directedness		
Level 1 Mildly inferential	202	8.2
2	459	18.6
3	1,125	45.4
4	495	20.0
5 Moderately inferential	104	4.2
6	60	2.4
7	20	.8
8 Highly inferential	9	.4
Total*	2,474	100.0
Dynamic Focus		
No Change in Focus	510	20.6
Reflections of Feeling	116	4.7
Requests for Additional Information	1,092	44.2
Dynamic Events—Past	151	6.1
Dynamic Events—Present	288	11.6
Therapist as Expert	284	11.5
Analysis of Transference	33	1.3
Total*	2,474	100.0
Initiative		
Level 1 Mild	170	6.9
2	387	15.6
3	1,086	43.9
4 Moderate	514	20.8
5	175	7.1
6	65	2.6
7 Strong	77	3.1
Total*	2,474	100.0
Therapeutic Climate		
−2	5	.2
−1	31	1.2
0	2,326	94.0
+1	108	4.4
+2	4	.2
Total*	2,474	100.0

* Excludes silences and unclassifiable responses.

picture of the patient's person and problem. Highly inferential communications as well as comments revealing a high level of initiative were relatively rare.

Experience in psychotherapy. It seems reasonable to hypothesize that a highly experienced therapist will conduct an initial interview differently from an inexperienced resident. Table 6 shows the results of the analysis of variance which may be summarized as follows:[9]

1. Inexperienced psychiatrists tended to ask more exploratory questions than experienced therapists.

2. Experienced psychiatrists tended to give a larger number of interpretive responses than inexperienced psychiatrists. These communications tended to be more inferential, and their dynamic focus concerned dynamic interpersonal events in the patient's past and present life.

3. Experienced therapists showed a higher degree of initiative in their communications than inexperienced practitioners.

4. Experienced therapists tended to change the dynamic focus of their communications more than the less experienced respondents.

5. Experienced therapists tended to be slightly "warmer."

The above changes appeared to be progressive with increasing experience.

Analysts, psychiatrists, residents. A parallel analysis was performed on a somewhat different criterion—the respondent's self-description of his status within the profession. The analysis is analogous to the first one because the three groups were markedly divergent in terms of professional experience: Analysts' mean experience was 13.7 years; psychiatrists', 7.3 years; residents', .9 years. However, the variable of personal analysis and analytic training seems to enter more prominently into this grouping. The first group (with one exception) had both personal analysis and analytic training; the third group, for the most part, had neither; the middle group was divided.

[9] Unless otherwise indicated, the reference to statistical results in this section is Table 6. The descriptive statements are based on statistically significant group comparisons, as shown in that table. When more than two groups were involved, an attempt was made to "locate" the significant over-all difference by comparing subgroups with each other. Such references in the text describe subsidiary comparisons which were statistically significant at an acceptable level. Occasional exceptions to this procedure are specifically noted.

TABLE 6

SUMMARY OF STATISTICAL ANALYSES*

Variable	System Components: Type: Silence	Type: Explorations	Type: Interpretations	Type: Direct Guidance	Depth Directedness	Dynamic focus: Maintain focus	Dynamic focus: Addit. Info.	Dynamic focus: Dynamic Events	Dynamic focus: Therap. as Expert	Initiative	Climate: "Cold"	Climate: "Warm"	Subgroups	N
Experience	—	.05	.01	—	.01	.05	.05	.02	—	.01	—	.09	0–1.75 yrs.	40
													2–9 yrs.	53
													10–30 yrs.	34
Grouping within Profession	—	.06	.01	—	.01	.001	—	.01	—	.02	—	—	Analysts	30
													Psychiatrists	57
													Residents	43
Personal Analysis†	.02	.02	—	—	—	—	—	—	—	—	—	—	Personal Analysis	28
													No Personal Analysis	28
Membership in Amer. Psy. Ass'n.	—	—	—	—	—	—	—	—	—	—	—	—	Members	18
													Non-Members	14
Length of Analysis	—	—	—	—	—	—	—	—	—	—	—	—	More than 500 hours	22
													Less than 500 hours	13
Theoretical Orientation†	—	—	—	—	—	—	—	—	—	—	—	—	Orthodox Freudians	16
													Neo-Freudians	16
Attitude toward Patient	—	—	—	—	—	—	.03	—	—	—	.07	.05	Positive (+)	39
													Neutral (0)	26
													Negative (−)	51
													Ambivalent (±)	13
Prognosis	.01	—	—	—	—	.05	—	—	—	.05	.05	.05	Favorable (+)	41
													Non-committal (0)	17
													Unfavorable (−)	58
Attitude-Prognosis (Jointly)	—	—	—	—	—	—	.05	—	—	—	.05	—	Att.: +; Prog.: +	22
													Att.; −; Prog.: −	33

* Significance levels (two-tailed tests) are based on the Mann-Whitney *U* test (comparisons of two groups) or the Kruskal-Wallis one-way analysis of variance (comparisons of more than two groups). Probabilities of .10 or less are given in the table; *p* values greater than .10 have been omitted (indicated by a dash). The statistic used for *Depth-Directedness*, *Initiative*, and *Climate* (over-all comparisons) was chi square.

† Matched samples.

(On an over-all basis, the correlation between professional experience and personal analysis was .43.)

The following differences were statistically significant:

1. The number of interpretive communications given by analysts was proportionately larger than that of residents. As expected, such communications tended to be more inferential.
2. The relative degree of initiative shown by analysts was greater than that of residents.
3. Analysts tended to shift the dynamic focus from passive acceptance to past and current interpersonal events more than psychiatrists and residents.
4. While the over-all comparison was nonsignificant, the relative frequency of responses whose emotional tone was judged "cold" was greater among analysts than among residents (ratio of 3:1). Differences among "warm" responses were not statistically significant.
5. The decline of exploratory communications with experience, reported above, was significant at the .06 level for the present grouping. The direction of change was identical.

Personal analysis. While the foregoing results suggest that the observed differences are primarily a function of experience, it is possible that personal analysis might make a contribution of its own. In order to explore this question, two samples were equated in terms of experience and at each experience level a therapist with a personal analysis was matched with a therapist without this training. The matched samples are described in Table 7. The results indicate that the only significant differences concern the relative frequency of silent responses and exploratory questions: analyzed therapists gave a significantly larger number of silent responses, this difference being achieved at the expense of exploratory questions.

TABLE 7
SUMMARY OF BIOGRAPHICAL VARIABLES FOR TWO MATCHED SAMPLES OF PSYCHIATRISTS
(N = 28)

Variable	Personal Analysis	No Personal Analysis
Age (mean)	38.4 years	38.2 years
Experience in therapy (mean)	6.4 years	6.4 years
Personal Analysis (mean)	458.8 hours	—
Analysts	10	1
Psychiatrists	10	17
Residents	8	10
Total Sample (M.D. degree)	28	28
Theoretical Orientation:		
Psychoanalytically oriented—general	19	15
Freud—orthodox	1	0
Sullivan	2	3
Meyer	0	3
Miscellaneous	6	7
Male/Female	24/4	25/3

To learn whether these differences might be more pronounced at the high or the low end of experience, the matched samples were equally divided into a high and low experience group. Comparisons showed that the difference in silent responses was particularly prominent among inexperienced therapists, suggesting that personal analysis might have a greater influence at the lower end of the experience continuum. These results are supported by another analysis in which 18 members of the American Psychoanalytic Association were compared with 14 nonmembers (both groups had a mean experience of approximately 14 years but most members of the second group had no personal analysis). No statistically significant differences were observed in these highly experienced groups. Nor did therapists whose analyses exceeded 500 hours differ in their responses from therapists whose personal therapy was relatively less extensive.

Theoretical orientation. Despite the difficulties of isolating "pure" groups, an attempt was made to match two presumably divergent samples: those who subscribed to an "orthodox Freudian" orientation and those who might be called "neo-Freudians." Each group included 16 therapists, all of whom had undergone personal analysis. The orthodox Freudians' median experience level was 8.5 years, median length of personal analysis 650 hours; the neo-Freudians had a median experience of 7.0 years, and the median length of their analyses was 250 hours. Statistical comparisons failed to reveal any reliable differences between these groups.

Attitude toward the patient. It was shown in the preceding section that the therapist's conscious attitude toward the patient tended to be correlated with his clinical evaluations as well as his treatment plans, goals, and proposed techniques. This finding suggested the hypothesis that the therapist's self-rating of his conscious attitude might carry over to his communications; specifically, that therapists expressing a negative attitude would tend to be "colder," that is, more rejecting in their communications than therapists professing a positive attitude.

Comparisons between groups of therapists professing a positive, neutral, negative, or ambivalent attitude disclosed a statistically significant difference relative to exploratory questions; however, since this finding was not corroborated on related system components, it may be considered due to chance.

With regard to *Therapeutic Climate*, chi square for "cold" responses was significant at the .07 level of confidence, for "warm" responses it was significant at the .05 level. Therapists indicating a negative attitude gave three times the number of "cold" responses as therapists having a positive attitude. The "ambivalent" attitude group was differentiated reliably from each of the remaining groups by giving the largest proportion of "warm" responses. The positive attitude group did not differ significantly from the negative one in terms of "warm" responses.

Prognosis. In view of the correlation between therapists' self-estimates of their attitude toward the patient and their prognostic ratings, a parallel analysis was conducted by grouping respondents according to their prognostic estimates. The results may be summarized as follows:

1. Statistically significant group differences were observed with regard to silent responses. Therapists expressing a noncommittal prognostic estimate tended to give the smallest number of silent responses.

2. Respondents giving a favorable prognosis were more likely to accept the patient's focus without attempting to shift it.

3. Therapists giving a noncommittal prognosis tended to show more initiative in their communications than the other two groups.

4. Therapists whose prognosis was unfavorable gave more than four times as many "cold" responses than respondents who considered the prognosis favorable. (The noncommittal group appeared closer to the "favorable" one.)

5. The relative frequency of "warm" responses was greatest among therapists whose prognosis was noncommittal, and relatively low (one-half or less) in both the "favorable" and the "unfavorable" groups.

Attitude and prognosis considered jointly. To cast further light on the above findings, two extreme groups were isolated and compared. The first group included therapists whose attitude toward the patient was positive and whose prognosis was favorable (mean experience = 5.6 years), the second those whose attitude was negative and whose prognosis was unfavorable (mean experience = 6.6 years).[10] The group comparisons revealed few technique differences, except that the second group tended to change the focus by asking significantly more exploratory questions. In terms of *Climate*, however, the differences reported separately for Attitude and Prognosis were accentuated. Only 1 therapist in the first group obtained a "cold" response, in contrast to 10 in the second group—a ratio of almost 1:7. "Warm" responses revealed a small difference in the predicted direction which was not statistically significant.

It may be appropriate to reiterate the earlier statement that the scores on *Climate* represent almost certainly a gross underestimate, so that many potentially "positive" or "negative" responses had to be treated as "neutral" for lack of sufficient evidence. Consequently, the frequencies involved in these comparisons are relatively small, but the relationships between film responses and therapist's attitude and prognosis nevertheless emerge as quite consistent. "Cold" responses, by this evidence, appear to be a more sensitive indicator of the therapist's conscious attitude than "warm," empathic ones.

[10] The mean difference was statistically nonsignificant.

Further exploration of the empathy variable

The findings highlighting the interrelationships between the emotional tone of therapists' communications, conscious attitude, and clinical evaluations led to further explorations. In scoring the responses to the points of interruption in the film, our[11] attention was called early to marked qualitative differences in terms of responses to particular choice points (some of which appeared especially critical) as well as the over-all response patterns. As we developed a greater appreciation for the range of responses, we became increasingly impressed by differences in therapists' underlying attitudes toward the patient, their willingness or unwillingness to engage in a therapeutic relationship, and their empathy. To be sure, some protocols did not lend themselves to this kind of analysis, mostly because the respondent had been unable or unwilling to make his stand sufficiently explicit, so that his written statements which at best conveyed only a pale impression of the sound of his voice left the question indeterminate. But a substantial number of protocols did provide sufficient evidence, and often very clear-cut evidence.

Responses to selected choice points. Before dealing with certain global impressions gained from complete protocols, I should like to present some examples of responses to "critical" choice points. Often there seemed to be a noteworthy consistency between therapists' responses to several of these choice points.

One of the most controversial and seemingly most diagnostic passages occurred at the end of the interview. Somewhat unexpectedly perhaps, the patient makes an impassioned plea for reassurance by expressing concern about his anxiety symp-

[11] I wish to take this opportunity to reiterate my sincere appreciation to Mrs. Rebecca E. Rieger, my research associate, who made a vital contribution to this phase of the work.

toms which, he fears, might lead to a heart attack. The question expresses both his obsessive ruminations and his intense anxiety. It may be a "loaded" question in the sense that his unconscious objective is to put the therapist "on the spot," to force him to commit himself. It also expresses the patient's dependency needs, his demands for a magical protector, and his passive-aggressive submission to an authority figure. The passage ends with the question:

> That's all I want to ask you. Are they [these attacks] going to kill me? Will they cause me to have . . . will they cause my heart to flop out from under me some day or will they damage my nerve tissue or even my . . . my thinking?

Here are some typical examples of therapists' responses:

> I feel that your emotional difficulty is really more important than any heart disease you might have. We can have you checked physically to determine your physical health.
>
> You prefer to talk of your symptoms—not your feelings. They are not the real problem.
>
> You must be convinced by now that they won't damage you after all these years. Tell me, how would your life be different if you didn't have them?
>
> No; but the attacks are causing you a lot of distress.
>
> No; these are only the effects of things you don't understand within yourself.
>
> What we can do here is see what we can find out about the attacks, what sets them off.
>
> No, your physical symptoms will not harm you. But it is important to understand what they do for you.
>
> No, this will not happen.
>
> I seem to feel that you know there is a connection between how you feel and the way you think life has treated you.
>
> Naturally, you are concerned about yourself.

In the following replies the therapist sidesteps the issue. He may feel that by doing so he is avoiding a trap, but in any event he is deliberately or unknowingly neglecting the patient's feelings. It is predictable that this evasion would be experienced by the patient as a rejection or at least as a "letting down." If it is kept in mind that the interview is drawing to a close, the possibility of increasing rather than allaying the patient's anxiety should be a matter of serious concern to the therapist. Operationally, such therapists either subtly changed the focus by asking a related question, or they threw the question back. The latter maneuver, used quite frequently by inexperienced therapists, appeared to be an evasion because the patient had talked about his feelings at some length.

> You say you have these attacks at home now?
>
> Even your thinking, huh?
>
> Why should they damage you?
>
> These are things you fear?
>
> I'd like to discuss the types of work that you have done, for a while.
>
> What did the other doctors tell you?
>
> How do you feel when you feel "safe"?
>
> What do you think?
>
> Do you think they will?

There was a small group of therapists who decided either to make no reply at all or to launch a frontal attack upon the patient's neurotic defenses:

> With your good luck, you're going to live and suffer.
>
> Do you want to be a chronic invalid and be taken care of?
>
> What difference would it make if I said yes or no? (Perhaps I might go on, not antagonistically, to explain that's not the problem.)
>
> They might.
>
> I don't know.

The following responses reflect the therapist's desire to avoid an authoritative answer and yet to give a forthright reply which may have the effect of allaying the patient's anxiety, at least temporarily:

> As far as I know that will not cause a heart attack or damage your nerve tissue, but you surely want more than this. You want help for these symptoms, and I'll have to know more.
>
> I know these feelings are very uncomfortable. I can't answer your question completely. I feel it's something that should be looked into.
>
> That's a very important question, of course, but something we'll have to go into fully later. First we need a fuller understanding of you and your problem. I'd like to talk with you again and go into everything in more detail. Let's make another appointment if it's all right with you.

These examples may convey something of the wide range of responses and their individual flavor. They also show how different therapists attempted to meet the problem inherent in the patient's concluding question. Apart from the patient's needs at the moment, it was clear that certain thera-

pists emerged as persons, as individuals, as human beings who were not averse to use the pronoun *I*, whereas others considered it important to remain anonymous. It was our impression that some of the most highly experienced therapists appeared most clearly as persons, both in an accepting and a rejecting sense; conversely, inexperience tended to go together with anonymity.

An interesting difference in responses was observed with regard to a passage in which the patient mentions a constructive striving toward health which, to the superficial viewer, might easily be considered subordinate to his recital of symptoms, etc. After detailing a number of symptoms, the patient says:

P.: You see what I mean? Pressure. Put your hand on your other hand; you got pressure. (T.: Mmm.) Well, that's just like it feels; that's the sensation of being hemmed in; you're cornered; you don't know which way to go. You are tired of the surroundings you are in and what you have been doing; you can't find anything that takes its place, that seems to be . . . to . . . fulfill the bill for you. (T.: Mmm.) But it's the sensations that worry me. (T.: Mmm.) This other stuff, huh, I can forget . . . (T.: Mmm.) . . . or have to if I want to regain my health.

The "obvious" thing to do at this point is to ask for further information:

Forget what?
What other stuff?

It seems to require greater alertness and self-awareness to focus on the patient's striving for health which appears heavily overlaid by his emphasis on symptoms:

You feel as if you'd like to be healthier, too?
You really want to get over these feelings?
How do you feel you can regain your health?
What kind of help might you expect from me?

Among therapists whose conscious attitude toward the patient (expressed independently in the questionnaire) was positive and who gave a favorable prognosis ($N = 22$), 32 per cent focused on the "health" aspect, whereas among the negative attitude—unfavorable prognosis group ($N = 33$), only 1 therapist (3 per cent) took this approach. Among the latter group, 12 therapists included the phrase "other stuff" in their responses, whereas in the former group only 2 therapists did, one of whom combined the remark with a reference to the patient's health. This piece of evidence suggests that the second group tended to be more preoccupied with "pathology" than the first one.

An illuminating contrast in approach is provided by therapists' responses to an episode in which the patient relates an experience of being discharged from a job. Comments encompassed empathic remarks about the patient's misfortune, his underlying feelings, requests for further information, and moral judgment or condemnation.

. . . So, when I got fired from the First National Bank in H——— for writing a little old stinking $35 check on my personal account and didn't have the money to cover it, I got fired for that . . . see . . . And in my home town, a thing like that gets around pretty quick, in a small community. I couldn't find a damn job. I didn't have any place to sleep. I didn't have a damn thing to eat. Being an Episcopalian, I went to a person who I knew . . . she ran a boarding house, and I chopped wood at night for my supper, see, to get something to eat. And that was the beginning of all my antisocial attitudes. And if that isn't three strikes against you before you come to bat, I'd like to know what in the hell it is.

You seem to have had a hard time of it, especially after your father died.
You feel badly about that time in your life.
I can see things were pretty rough for you.
You seem angry when you talk of this.
Tell me about these antisocial attitudes.
Despite the rough time, you seemed to know how to get work and take care of yourself.
How do you think the bank manager should have handled the situation?
How did the bank happen to fire you for what seemed just like an error?
Fired for writing a bad check, huh?
What would you have the bank do?
You did write the check and expected to get away with it?
You feel sorry for yourself; no one seemed interested.

Comparisons of responses to single items (stopping points) were often less revealing of the therapist's attitude toward the patient, but the totality of the response pattern frequently provided important clues. On the basis of these observations it was decided to attempt an over-all evaluation of the protocols.

Over-all evaluation of protocols. The objective of arriving at a global judgment of the degree of empathy shown by therapists was to determine whether such ratings could be made reliably, that is, whether independent raters could agree on the evaluation, and to explore the relationships between over-all ratings to other relevant therapist variables. Considering the exploratory character of this analysis, we restricted ourselves to a few gross categories. The major criteria are given below:

Score +: Therapist recognizes the existence of an emotional problem and communicates such understanding to the patient somewhere in the interview. Such communications may convey reassurance but this may not necessarily be the case.

Therapist recognizes that the patient is seeking help, that he is in need of help, that psychotherapy can help him, that there may be a relationship between patient's physical symptoms and his emotional conflicts, and that insight or understanding of the latter may alleviate the former.

Therapist communicates that he understands that the patient is suffering, that he is anxious and in a state of discomfort.

Therapist attempts to alleviate the patient's discomfort in the interview by putting him at ease, by trying to alleviate his anxiety, by instilling some hope for the future, by recognizing his past accomplishments despite difficulties, by raising his self-esteem.

Therapist feels the need to communicate to the patient something about treatment plans, referral, etc., that is, he recognizes that the patient has a right to expect a statement or recommendation from the interviewer as an expert, that the interview must serve a useful purpose to the patient, either by opening further possibilities or by making certain that the present experience is a constructive one.

Therapist gives evidence of listening to the patient's story, either by pertinent remarks or questions or by respectful silence. There should be no evidence that therapist changes a topic in order to impose his own frame of reference or trend of thought (especially in the beginning), nor should he respond with silence to urgent pleas from the patient.

The phrasing of comments should be respectful, nonderogatory, sympathetic, noncritical, nonjudgmental. There should be a complete absence of communications which by themselves would be assigned a score of −.

Score −: Therapist is cold, distant, unperceptive, unfeeling, and impersonal. Such attitudes would be inferred from a total absence of communications receiving a score of + or, more directly, from overt evidence: communications which seem to imply derogation, criticism, antagonism, hostility, disrespect, moral judgment, condemnation, accusation, impatience, punishment, nonacceptance of the patient as a person, or rejection. Included also would be communications by which the therapist implies or states openly that the patient is malingering, that he is anti-social, lazy, derelict in his duties to his family, exploiting, manipulating, sponging on others, psychopathic, unconventional, "crazy," ridiculous, silly, exaggerating, falsely accusing others, or imposing on the interviewer in any manner.

Therapist devotes large portions of the interview to gathering facts about the patient's life history. The interview resembles a cross-examination.

Therapist "gives" nothing to the patient such as: recommendations for treatment, recognition that a problem exists, that psychotherapeutic help is available, that problems like his can and have been helped by psychotherapy; reassurance; acknowledgment that the patient is having a hard time, that his emotional and/or reality problems complicate his living.

Therapist responds with silence to patient's plea for reassurance, or uses rote devices to throw back the question. Therapist fails to respond to patient's underlying feelings throughout the interview.

Therapist gives interpretations which in the context of an initial interview and the patient's emotional state seem premature, inappropriate, etc. This judgment would apply with particular force to interpretations which are given very early in the interview, confront the patient directly with an important aspect of his problem, are not stated tentatively but rather authoritatively; which because of their directness would be experienced by the patient as an attack on his self-esteem and consequently rejected; interpretations which seem "over the patient's head."

Score 0: The record provides insufficient evidence to warrant either a score of + or −, either because communications are minimal or because their emotional tone is "neutral." Therapist does not emerge sufficiently to be judged on the aforementioned criteria. (Records receiving a score of 0 tend to lean toward the − side, principally because there is an absence of positive indicators.)

Score ±: The record gives evidence of both + and − indicators, as previously defined, and it is difficult to determine which of these preponderate.

Rater agreement. Two successive ratings were made by two trained raters working independently. Rater agreement is reported in Table 8. Since there were indications of a certain affinity between + and ± ratings, and between 0 and − ratings, differences within these groupings were counted as "partial agreement." It is believed that sound recordings of the therapist's voice

TABLE 8
AGREEMENT WITHIN AND BETWEEN TWO INDEPENDENT RATERS
(N = 131)

	Full* Agreement	Partial† Agreement	Disagreement
Rater 1: Rating 1 vs. Rating 2	69 (53%)	35 (27%)	27 (20%)
Rater 2: Rating 1 vs. Rating 2	52 (40%)	38 (29%)	41 (31%)
Rater 1 vs. Rater 2: Rating 1	51 (39%)	33 (25%)	47 (36%)
Rater 1 vs. Rater 2: Rating 2	57 (44%)	28 (21%)	46 (35%)

* using four categories: +, −, 0, and ±.
† combining categories + and ±, and 0 and −.

would have substantially increased the reliability of the judgments.

Relationships of empathy ratings to other variables. The four empathy ratings were combined into a single rating, and compared with therapists' self-ratings of their *attitude* toward the patient. Chi square was significant at the .02 level of confidence. The distributions indicated that almost two-thirds of the therapists expressing a positive attitude received positive empathy ratings; negative attitude tended to be related to negative empathy ratings in approximately the same manner.

A similar analysis relating empathy ratings to therapists' *prognostic estimates* yielded a chi square which was significant at the .05 level, signifying an association between favorableness of the prognosis and greater empathy.

There was no statistically significant association between empathy ratings and *experience* in psychotherapy. A very slight trend (significant only at the .13 level of confidence) indicated that therapists having 10 or more years of experience tended to obtain a larger proportion of positive ratings.

When empathy ratings were related to the samples matched on *personal analysis*, the results were strongly in favor of the analyzed group, as shown in Table 9. Table 9 also indicates that the distribution of empathy ratings shows no differentiation in the less experienced groups, but the differences become greatly pronounced in the more experienced groups. Experienced nonanalyzed therapists had more than three times the number of negative empathy ratings as experienced analyzed therapists.

A further breakdown in terms of the therapist's self-rating of his attitude was undertaken in order to explore possible relationships between empathy and attitude. The results indicated that on an over-all basis the nonanalyzed group somewhat exceeded the analyzed group in terms of negative attitudes, but the difference was not statistically significant. In the less experienced group, analyzed therapists tended to have a more positive attitude; however, in the more highly experienced group the trend appeared to be almost reversed. Although these findings must be considered highly tentative because of the small size of the subsamples, in the experienced analyzed group a conscious *negative* attitude appeared to be associated more often with a *positive* empathy rating, whereas in the nonanalyzed group there appeared to be greater congruence between negative attitude and lack of empathy. Moreover, no experienced analyzed therapist having a positive attitude obtained a negative em-

TABLE 9
DISTRIBUTIONS OF EMPATHY RATINGS AND SELF-RATINGS OF ATTITUDE IN TWO GROUPS OF PSYCHIATRISTS, MATCHED IN TERMS OF PERSONAL ANALYSIS

Experience in Therapy	Attitude toward Patient	Personal Analysis			No Personal Analysis		
		Empathy Rating			Empathy Rating		
		+	−	Total	+	−	Total
0–3 years	+	3	6	9	2	2	4
	−	4	1	5	4	6	10
Total		7	7	14	6	8	14
3.5–30 years	+	4	0	4	2	4	6
	−	7	3	10	2	6	8
Total		11	3	14	4	10	14
Combined	+	7	6	13	4	6	10
	−	11	4	15	6	12	18
Total		18	10	28	10	18	28

pathy rating; in contrast, such a discrepancy was observed among four members of the nonanalyzed group.

The data suggest that at the higher levels of experience a therapist whose training had included personal analysis was better able to empathize with his patient regardless of his (the therapist's) conscious attitude, or perhaps in spite of a conscious (negative) attitude. Contrariwise, a nonanalyzed therapist having a negative conscious attitude toward his patient was more likely to communicate this attitude to the patient. Furthermore, experienced nonanalyzed therapists were more prone to profess a positive attitude even though their communications to the patient revealed a lack of empathy, while no experienced analyzed therapist claimed a positive attitude that was not associated with empathy. In other words, personal analysis seemed to have little effect on the therapist's conscious attitude toward the patient—a majority in both groups admitted a negative reaction—but it had a differential effect upon the degree of empathy communicated to the patient.

DISCUSSION

The findings have numerous implications for psychotherapy. The importance one attaches to the implications is, of course, contingent upon the extent to which one can generalize from an experimental situation to the "naturally occurring" events in the interview process. It is not contended that the behavior of the psychiatrist in an initial interview with a "real" patient bears a one-to-one relationship to his performance in the experiment—an objection sometimes raised by respondents. What is asserted is that the evaluative processes, as they were studied under experimental conditions, are sufficiently similar to the "normal" behavior of the therapist to generate important hypotheses about his behavior in a first interview. Hypotheses thus derived are valuable if they raise further questions for research, and if they can be applied to practice and training in psychotherapy.

Two broadly defined groups of therapists appear to emerge from this investigation, whose attitudes and approach may be characterized schematically in this manner:

Group I	Group II
Therapist's Attitude toward Patient	
Somewhat Positive	Somewhat Negative
"Reason"	
"He is suffering and needs help."	"Unpleasant personality characteristics," poor risk, poor motivation for therapy
Diagnosis: (preferred)	
Hysteria (anxiety hysteria)	Paranoid, character disorder, psychopath, anxiety
Clinical Evaluation:	
A great deal of anxiety	Slightly less anxiety
Relatively more insight	Relatively less insight
Relatively more capable of self-observation vs. rationalization	Relatively less capable of self-observation vs. rationalization
More mature emotionally	Less mature emotionally
Fairly inadequate social adjustment	Very inadequate social adjustment
Prognosis with therapy:	
Favorable	Unfavorable
Treatment Plans:	
	Avoid dealing with sexual impulses, hostility, anger, death wishes, unconscious material.
Danger in "pushing" patient too fast. Therapist must be sympathetic, understanding.	Danger of therapist's irritation, anger, impatience, other countertransference reactions.
Handle countertransference by being aware of it or handle in therapy.	Handle countertransference by discontinuing therapy or not accepting patient at all.
Would not make recommendations to the patient about his mode of living. This is not the problem in therapy.	Might make recommendations to the patient to change his present mode of living or his environment.
Would tend to be permissive with patient.	Would tend to be strict with patient.
Would tend to be passive in therapy.	Would tend to be active in therapy.
Would encourage free association.	Would discourage free association.

Treatment Plans:	
Recommend intensive (insight) therapy.	Recommend supportive therapy, group therapy, drugs, other adjuncts.
3 or more weekly interviews	1 or 2 weekly interviews
Symptom relief expected in 1 year or less	Evasive about length of time needed to accomplish symptom relief.
Would attempt relatively greater changes in patient's character structure.	Would attempt relatively little change in patient's character structure.
Therapist's experience:	
Relatively low	Relatively high

The above relationships are by no means invariant; we are dealing with *statistical trends*, and there were many subgroups of therapists who did not follow the pattern. However, the conclusion is suggested that clinical impressions and therapeutic planning are influenced by attitudinal variables within the therapist. Group I therapists appear to be more tolerant, more humane, more permissive, more "democratic," and more "therapeutic." Group II therapists emerge as more directive, disciplinarian, moralistic, and harsh. This contrast suggests the hypothesis that Group I therapists are "warmer" in their communications to the patient and that "cold," rejecting comments will be less frequent.

What is meant by this distinction? On the one hand, it is a basic attitude of understanding, respect, and compassion—what Albert Schweitzer calls "reverence for life." It is the ability to listen to the patient's story without preconception, prejudgment, or condemnation. It is the ability to pierce the neurotic distortions, the socially unacceptable attitudes and acts, the more unsavory aspects of his personality, and to see behind it a confused, bewildered, and helpless individual trying to shape his destiny, hampered and hindered by his neurotic conflicts and maladaptations. On the other hand, it is an attitude of coldness, calculation, "clinical evaluation," distance, "objectivity," aloofness, moral judgment, and condemnation. It is a readiness to take the neurotic defenses and the patient's character structure at face value, and to react to them with irritation, impatience, annoyance, and anger. It also is an attitude of forming a judgment about the patient's illness almost from the beginning of the interview, often accompanied by a diagnostic label of "psychopathic," "paranoid," etc.

If further evidence can be adduced that motivational factors within the therapist are related to his perceptions, evaluations, and techniques with a given patient, we would be faced with many more problems than can be explained by the empirical data now at our disposal. For example, a therapist who finds himself responding negatively to a patient in an initial interview, whether or not he is aware of the underlying reasons, might have to be particularly circumspect about his emotional reactions lest they have a detrimental effect upon his evaluations and therapeutic interventions, and he may have to ask himself whether he can be therapeutically effective under these conditions. A therapist whose initial attitude is neutral or positive, needless to say, may not be free from countertransference distortions either, but to the extent that he feels free from an immediate avoidance reaction he will—at least initially—have fewer problems in treatment than one who from the start has to deal with a problem within himself. It remains to be explored whether the rather large-scale avoidance reactions are specific to this patient, whether they occur more frequently with certain kinds of patients, and if so, what patient attitudes are principally responsible for a negative or positive trigger effect. It is not inconceivable that culturally determined stereotypes have a more pervasive influence upon the alleged nontreatability of certain behavior disorders than has hitherto been realized; and that these stereotypes may exert a vitiating influence upon the treatment of other conditions to which the stigma of social reprehensibility may become more subtly attached.

Glover (cited by Kubie, 2) has drawn

attention to the observation that analysts sometimes seem to achieve their greatest successes when they are beginners, to which Kubie adds his disappointment in a youthful expectation that increasing analytic experience would yield a higher percentage of therapeutic successes and a more precise understanding of how successes are achieved. These comments appear pertinent to the finding that, in the psychiatrist group, therapists professing a negative attitude toward the patient were significantly more experienced in therapy than those holding a positive attitude. The last named group, as a matter of fact, ranked lowest in experience —lower than either the ambivalent or the "neutral" group (which had the highest level of experience). Length of experience, on the other hand, was not significantly correlated with prognosis (although the trend was in the direction suggested by attitude) or other clinical evaluations, nor with length or other aspects of treatment. It was shown, on the other hand, that the quality of the therapists' dynamic formulations or their formulations of the course of the transference (also in part a qualitative judgment) did correlate with experience. We may wonder, therefore, whether inexperienced therapists are more accepting of the patient because of their youthful enthusiasm, or whether the more experienced practitioners are more disillusioned, more discouraged—or simply more realistic. The present data, unfortunately, do not permit a definite answer; they do support, however, the relevance of Kubie's thoughtful question.

The analyses suggested that the empathic as well as the unempathic approach was in part a reflection of the therapist's *conscious* attitude. That is, to some extent at least, therapists were aware of their positive or negative reaction to the patient and their willingness or unwillingness to enter into a therapeutic relationship with him. Undoubtedly they were less aware of the manner in which their attitude interacted with their clinical evaluations. In the light of this evidence, is it possible that the therapist's attitude, as conveyed by his communications to the patient, tends to bring about a realization of the therapist's expectations? For psychotherapy, the crux of the matter is not the perceptions and clinical evaluations or even the therapist's conscious attitude toward the patient; it is the manner in which these variables influence and structure the therapeutic relationship. This is one of the important problems requiring further exploration.

Consider, in this connection, the essentials of psychotherapy, as described in an essay by Masserman (3):

> The warm, unashamedly personal acceptance of each patient not as another "interesting case" in support of some preconceived theory, or as an object of inexorable analysis, or even a grist for another therapeutic mill, but as a hurt, frightened, and troubled human being seeking relief and guidance. . . .
>
> . . . the first objective will be *to help the patient recognize that his previous patterns of behavior were neither as necessary nor as advantageous as he had implicitly assumed them to be*. . . . It is . . . equally necessary *to utilize optimal transference situations and other therapeutic opportunities to impart the second essential portion of the dual dicta of insight: namely that new patterns of conduct are really preferable* . . . Such reorientations . . . may be rendered more effective by . . . Re-education of the patient by the therapist through reason, demonstration and, implicitly or explicitly, personal example. (Italics in the original.)

On this basis, the therapist's contribution to the treatment process is a dual one: it is personal and technical. His personal attributes (maturity, warmth, acceptance, etc.) enable him to create the kind of interpersonal relationship in which constructive personality change can take place; his knowledge of psychodynamic principles and techniques permits him, in and through this relationship, to initiate the kinds of emotional unlearning and learning experiences that are considered necessary to the alleviation or resolution of neurotic conflicts. The latter would be impossible without the former; the former, by itself, would never be sufficient. To recast this in the form of a hypothesis:

A. Psychotherapy is maximally effective when the therapist is able (*a*) to relate to the patient in a warm, empathic manner, so that the person of the therapist, as

revealed in this relationship, will in time serve as a new, more mature, and more desirable model of reality than past interpersonal relationships which have distorted the patient's perceptions of himself and others; and (*b*) by appropriate technical devices (interpretations, etc.) to demonstrate, clearly and effectively, the self-defeating character of the patient's previous patterns of interpersonal behavior. (The formulation, timing, and communication of these insights requires consummate skill which is probably best acquired through a combination of intensive personal analysis and supervised experience.)

B. Psychotherapy is less effective when only the first condition is met.

C. Psychotherapy is least effective when the first condition is lacking, irrespective of the status of the second condition.

I sincerely believe that the cause of psychotherapy as a scientific procedure for helping people with their neurotic conflicts will be advanced as we succeed in refining and testing this hypothesis.

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(Manuscript received July 3, 1957.)

I may be obsessed, or suffering from anthropocentric illusions, but I cannot escape the feeling that the human mind and human curiosity are significant in this world—even perhaps in the cosmos of geological time and intergalactic space. With this impression (or illusion) that the mind is the best of us, and the best of biological evolution, I cannot escape (and neither can you!) the feeling of a responsibility to glorify the human mind, take it seriously, even dream about its ultimate flowering into something far beyond the primitive muscle-guider and sensation-recorder with which we started.

—HARLOW SHAPLEY

COMMENT

Explorations in the Realm of Organization Theory: I. Models for Man

THERE is as yet no single formulation for organization theory. It is made up of a loose collection of systems—some of them imposing, generalized, and vague while others are small, precise, and quite powerful within the region for which they apply—in scores of different scientific languages. One feels that they have characteristics in common, but these relationships are only now being brought into perspective.

When searching for the elements to a unified theory which does not yet exist the explorer must carry with him certain criteria of interestingness. He hopes to make a contribution by placing in juxtaposition certain surprising findings which would never, in the normal course of contemporary investigations, have encountered each other and by this act promote a synthesis a few months or years before it becomes perfectly obvious. An explorer of organizational phenomena will be unmoved by ideas for constructing faster and larger computers, or for expanding input and output capacity, for reducing the frequency of error or for expanding the memory—unless an improvement of one or two orders of magnitude is promised. He is really seeking suggestions which seem to transform one mechanical or symbolic system into another which is more nearly in his own image. An organized system is one that behaves in a manner analogous to oneself, or to any of those institutions in which one participates.

The guiding questions may be put in the following form: How would a machine be constructed which could communicate with humans at more than one level of abstraction? For example, how could a machine be coded to search a library for materials which are of value in treating a given problem, so that the strategies employed are the same as those of a sophisticated scholar? Such a question immediately generates some subsidiary questions about theories for efficient search. In quite another direction one may wonder about trigger or amplification subsystems so constructed that a single decision in an organization can thereby result in a significant impact upon the immediate environment. The potential payoffs are readily apparent, but what are the costs necessarily associated with such subsystems? What alternatives for reducing cost have been devised by existing organizations? There will be many more questions on this level.

Seldom is it possible for the investigator to escape from the clutter of data and concern for the last tortured hypothesis and soar to heights where these matters can be seen in their perspective. Most of us never seem to achieve these flights under our own power, so it is greatly appreciated when this intellectual adventuring can be given an assist. Sometimes the high points occur in conversations, when all of the conditions for interaction seem to be just right. On a few memorable occasions a lecturer is able to achieve and hold a peak in the communication of the most sophisticated ideas. However such experiences, although treasured, are evanescent. A book that does not go stale is much less vulnerable to coloration by the emotions. It can be a base camp for expeditions.

Certainly the most fertile mind intensively investigating the properties of organization at the present time is that of Herbert A. Simon, presently at the Carnegie Institute of Technology in the Graduate School of Administration. His work began with a classic non-mathematical analysis of human behavior in an organizational context (*Administrative Behavior*, Macmillan, 1947, second edition, 1957). The recently collected and annotated articles constitute a new summit in the exposition of behavioral science that has appeared to date.[1] It is in a form where one can go back to it again and

[1] Herbert A. Simon, *Models of Man—Social and Rational*. John Wiley and Sons, New York, 1957.

again to pick out features at the horizon which had been missed in earlier glimpses. Upon closer inspection of the terrain we may decide that one or another view was a mirage, but in this collection one gets a feeling for the efficient method for exploration in the realm of organization theory.

Mathematical Descriptions of Human Behavior

The articles are not new—although the last of them was published as recently as September, 1956. For a good many years now I have kept close to the top of the stacks on my desk a brown paper envelope containing Simon's reprints along with the discussion papers that preceded them by a year or two. Items in it were being continually borrowed and returned, and the transactions occasionally led to discussions as to where this series of essays would lead. Simon seemed to be reaching the top of a peak with each major effort and then drifting on to some other kind of challenge. The first might be in economics, the second philosophy, the third statistics, then political science, social psychology, learning theory, and operations research are represented. Except for the very general theme of human behavior there seemed to be no connection between them. The book changes all that. The framework is now assembled but the thread that ties them together is not exposed. Indeed, the ordering is left as a minor mystery which can be solved by reading through to the very end. Simon says:

> The advantage of retaining the essays in their original form is that they stand on their own feet. The reader can begin where he will and omit what he will. My purpose, of course, in binding them together in a single volume is to seduce him from the familiar to the strange, from the simple to the involved. But given the multiplicity of starting points, it would be somewhat presumptuous for me to do much more than suggest a preferred order and show the logical relationship among the essays when they are arranged in that order.
>
> I shall be disappointed if some readers do not come, sooner or later to prefer *my* order. It is an order which plays pretty fast and loose with traditional boundaries but provides, I am convinced, a useful framework for deepening our understanding of the nature of man.

In order to scale the really impressive peaks one needs special climbing equipment and the skill to use it. In behavioral science the special equipment required is mathematics. These essays use mathematics as a matter of economy in formal argument. But it is utilized with discretion. This is not climbing for the thrill of it, as is evident in so much of pure mathematics and not a small amount of theorizing in philosophy, statistics and econometrics. It is explicitly aimed at achieving a new line of sight, at extending the horizon for analysis. Therefore the mathematical approach that is chosen in each context is not necessarily what is fashionable, nor what is esthetic (although it often has beauty), but only that which enables him to make the precise point intended. The attitude he expresses in this respect is revealing:

> It is a common whimsy among mathematicians to preface their books with the statement that the reader needs no particular mathematical training as prerequisite to the work in question but only a certain amount of "mathematical maturity." Since my own formal training in mathematics stopped somewhere short of the calculus, I was more than once the victim of such statements until I learned how deceptive they were and subjected myself to some hard study of mathematical textbooks—in the proper sequence and not failing to work out the exercises. "Mathematical maturity," it would seem, is acquired largely by studying mathematics.
>
> Most of these essays will be accessible to anyone who has had a good introductory course in the calculus. Some knowledge of modern algebra would be useful, particularly the elements of matrix theory and of the algebra of classes. The reader may need to refer to an advanced calculus text for some specific topics—partial derivatives, Lagrange multipliers, Laplace transforms, and simple differential equations.
>
> And a final warning. It is currently fashionable among mathematicians to play down the importance of the calculus, and to stress set-theoretical methods and abstract algebra. This point of view is associated with a taste for game theory and a predilection for probabilistic models in preference to deterministic models. The issues are too involved to be discussed here, but the aspiring mathematical social scientist will do well to acquire some thorough training in the calculus early in the game.

This reviewer, alas, has not gone through all this mathematical exercise. He is restricted, like the great preponderance of readers, to scrambling, rather than scaling, although he will occasionally assay a few of

the easy ascents. However, most of the basic ideas arrived at are still accessible, although by roundabout pathways, and Simon has been considerate enough to provide a guide for the approaches. The four divisions in this book are formal extensions of portions of an earlier nonmathematical volume (*Administrative Behavior*).

Very likely too much attention has been given here to the mode of analysis. Now let us gauge the substance. The intention is to capture the central idea in each of the models that is taken up, comparing it sometimes with one or more alternative approaches, and finally lead into the discussion of the further experiments and explorations that are generated as a consequence of such theorizing. For these purposes a superficial discussion of context and "general idea" often will not do, because the central theme would be misrepresented. Therefore, over some stretches that follow, words must be used with a precision and austerity approaching that of mathematics.

Causation and Influence Relations

These papers, five in number, are intended to be a reconsideration of the central problem of politics—that of power, or directed influence relations—but Simon begins his analysis by digging at the roots of causality. When can one be said to have brought about, or caused, a social event? We know that organizations continue to exist because a restricted set of actions and events are within their power to control to their advantage.

Causality provides some special puzzles in social science. The arguments of David Hume on this subject (that *connections* between events cannot be perceived—only deduced through some prior theory) have not been gainsaid—yet all of us still proceed to simplify the world by constructing patterns of causal relations between recurring events. If A and B are functionally related in their appearance, and A precedes B, it can be presumed that A causes B. The significant feature here is the asymmetry between A and B. This aspect can be used for the creation of a specialized language for description used by a hypothetical observer who experiments to see what happens in the system of relations under scrutiny. The observer may manipulate parameters (exogenous variables) in search of invariant conditional relations, seeking those which are uniquely causal. I shall not try to reproduce here the rather involved, somewhat constrained definitions of causation that resulted. Simon does manage to avoid one of the prime objections raised against earlier formal definitions—the reversal of the ordering through negation.

The model for the exercise of influence is an extension of the formalizing labors of Lasswell and Kaplan (in *Power and Society*). The influence of A upon B is assumed to be a one-way coupling over the very short run. However when the processes of influence operate over time—the normal case—we may expect that B will generate some influence, perhaps of a different kind, upon A. Then things become more complicated. The *expected* reaction of B will often affect the choice of the *original* behavior of A and the time lag in the causal relation has, for all practical purposes, vanished! Nevertheless an asymmetric relationship still exists, and its dimensions can be estimated. For example, Simon asserts that the influence of any political or administrative position (A) is proportional to the amount of change induced throughout the system by a change in the characteristics of the individual occupying the position in question.

The *influence base* (wealth, "connections," etc.) is a rather static measure of influence. However influence may be used either to expand the base ("the rich seek to get richer") through an investment process, or it may be expended as a means of securing other values. The gerrymandering of legislative districts is an example provided for the "investment" of power, because it virtually insures continuity despite inadvertent losses in public support. The passage of some controversial piece of legislation at the cost of future support from fringe adherents or with the risk of splitting into factions may be considered to be an "expenditure" of power.

Despite these properties, influence re-

mains a slippery evanescent phenomenon, without the simple additive properties of money. Perhaps that is why so many Latin American dictators convert it into something more tangible—such as Swiss securities and New York bank accounts. If B_1 discerns that he, in concert with B_2, $B_3 \cdots B_1$, can change the response to A with impunity, A's influence is destroyed—a revolution occurs. When the asymmetric relation is fragile we move from causation to game theory as the preferred method of analysis, an approach which is postponed in the book because of the inherent difficulties.

Simon's concept of *authority* in this model is simple and satisfying. B accepts authority when his choice among alternative behaviors is determined by the communicated decision of A. The authority of A is limited to a certain *zone of acceptance* of B. Under these circumstances, the authority of A (which is only a part of his influence, because A may use more devious methods of persuasion which makes B feel that the decision was really his own or A may manipulate the environment of B or the latter's relations with others) may be measured by the range of behaviors B will perform at A's command that are different from his behavior when independent of A. This range of behaviors may be said to have a "size" which may increase or shrink, but is likely to undergo drastic change if potential resisters of authority manage to combine.

Suppose, to the extent that this is possible, these influence relations should be assessed and publicly reported by the social scientists —would this bring about a notable change in the influence relations? Simon comes to the conclusion that it would not in a two-party election (unless virtually the whole electorate hears of the "prediction" and most of it succumbs to an "underdog" impulse) but different outcomes could easily arise when the choice is between three parties or more.

How far does this get us? What is the unfinished business in the study of power, authority, and influence? I find that a great deal of thought has been applied to the asymmetric relationship of A to B but not to the influence of A upon a relationship of B with others, who may or may not be under the influence of A. The difference between a perfunctory organization and a crack outfit is determined largely by the latter's capacity to manipulate internally these second and higher order relationships. Simon gets at these problems in his next section, but from a wholly different perspective—because in this area he must address himself to questions taken seriously by social psychologists and sociologists.

Social Processes

In this case one is no longer concerned with history, philosophy, and the observation of political processes, but with *experiment*, as tempered and guided by observation. Experimental work on small groups in different contexts has made possible a number of rather carefully framed verbal generalizations of the phenomena. Simon has sharpened these systems a good deal more, detecting more than a few misinterpretations of the data, in the process of setting them up as mathematical models.

However the experiments are far from complete—ways are continuously being discovered for laying open to view the interpersonal reactions within a group or team—so we do not know whether this particular choice of variables will be strategic for the long run. The problems of measurement very quickly reach dimensions that exceed the capacity not only of present computers, but those we hope to have in the proximate future. The connection between theory and experiment in this area is beginning to assume the form that has already been taken in biophysics, genetics, etc. It was in this area, for example, that Guetzkow, Simon, and collaborators reported the first complete replication of a sociological experiment in 1955 (that of Bavelas and Levitt) with appropriate extensions and independent confirmations.[2] After some years of experiment and analysis have accumulated it is anticipated that we shall have a quite general theory for human behavior in

[2] H. Guetzkow and H. A. Simon, "The Impact of Certain Communication Nets Upon Organization and Performance in Task-Oriented Groups," *Management Science*, 1, 233 (1955).

groups, and the three different models presented here would very likely be special cases of that theory. It seems best to postpone discussion in this particular area.

The last paper in this group treats of social processes that can be represented as stochastic processes, where the probability that the next act will be of a certain kind depends upon the action that has already taken place—certainly a characteristic of much of organized behavior. If the stochastic process is appropriately defined one obtains frequencies of actions which have the same relationships to each other as those which were pointed out before World War II by Zipf; if these actions are defined as words used in written communications, and the respective words employed were arranged in a list so that words were ordered in terms of frequency of occurrence, and the logarithm of the rank on this list were plotted against the logarithm of the frequency, the points fall remarkably close to a straight line—thus signifying that a significant relationship very likely exists. Another way of seeing this relationship in the real world is by noting that the second largest metropolitan area in a large economy is about half the size of the largest, the third largest is about a third that size, and the fiftieth city about one-fiftieth that size. In the case of cities, the social actions involved are largely the decisions of migrants as to where they should settle. These relationships are of course not precise but the tendency is so strong that one begins to look for principles underlying the growth of cities which may be expected to result in the phenomenon.

Mandelbrot had attempted to explain this phenomenon for language by assuming that the frequencies of words are determined by the users so as to maximize the bits of information transmitted per symbol.[3] He wanted to assign probabilities of usage to printed (or spoken) words so that their total cost will be minimized, while keeping the rate of communication (in information units) constant. There seem to me to be ways of extending his approach to the growth of cities. Simon's treatment is quite different—he assumes words are selected for communication purposes according to a joint probability of imitation and association with prior words. His model involves averaging rather than optimizing, on the belief that the optimizing is done when the correct word is chosen for the immediate context but the frequencies of word usage are drawn from thousands of different contexts.

It seems to me that there is missing in this emphasis upon stochastic processes, and their effect upon word frequencies, any place where expectations for *future* conditions influence the choices in the present. In the case of written materials, to which Simon, Zipf, and Mandelbrot have addressed themselves in entirety because reasonably good statistics existed, the author and the editors have been over the whole text before; they have made assessments of both the future and the past of the reader when he is at a given stage in the work and have revised, polished, and edited the manuscript so as to conform to the needs of a standard reader they had in mind. The present and the immediate future of the reader of the work has been "fixed." The words he is reading now are as much conditioned by what words appear in the next few sentences and paragraphs as by what has gone before. Therefore, in this context, teleological approaches in theory building seem quite valid. There seem to be some interesting possibilities for analysis in this added insight. For those of us who employ models for generating hypotheses about phenomena in the real world there is now, in this particular area, an embarrassment of riches.

Motivation: Inducements and Contributions

Under this heading Simon moves from models of social man to other models which

[3] B. Mandelbrot, "An Information Theory of the Statistical Structure of Language. . ." in *Proceedings of a Symposium on Applications of Communications Theory—1952*. Butterworth, London, 1953. For a discussion of these relationships which is simpler to follow see Colin Cherry's *On Human Communication*, Technology Press and John Wiley, Cambridge, Massachusetts and New York, 1957; also Anatol Rapoport, "The Stochastic and 'Teleological Rationales' of Certain Distributions and the So-Called Principle of Least Effort," *Behavioral Science* 2, 147 (1957).

incorporate a view of economic man. Instead of "groupiness" or "organization loyalty" for its own sake (where the organization is also providing something else in a material sense that is relevant to personal needs but the implications are left unspecified, as is appropriate in models of social man) we take up the use of teams and organizations for the achievement of ends which cannot otherwise be reached by the individual. He *must* cooperate in order to get these things for himself. The group is then an instrument of the individual, and the latter is more or less indifferent to his task and role in the organization so long as he obtains a fair allocation of the reward.

In the first paper of the two presented I find concepts and arguments so striking that I have had occasion to refer to them several times a year since they were originally published. In it the pure theory of the firm as developed in economics is compared to a theory of organization which has been developed for corporate entities and bureaucracies. In the classical theory of the firm the decisions are guided so as to maximize profit. In the theory of organization, the decision makers for the organization are primarily concerned with its survival. The *firm* will be concerned with the marginal utility of extra participation, while the *organization* makes its computations in terms of average utilities ("all or none" participation or membership). Recent directions of the theory of the firm in economic journals (monopoly-monopsony, game theoretical approaches, and oligopoly considerations) offer a compromise, while the consideration of the organizational relations themselves as economic assets is a compromise made from the other side. The organization that exists in the real world may be represented as one or another form of hybrid between these two idealizations. The second paper extends this mode of thinking to the employment contract.

The usefulness of this mode of organizational theorizing, in contradistinction to the economic thinking that is applicable to the same range of problems, was emphasized very recently in a short paper[4] concerned with the explanations for the pay scales of executives. Economists would assert that executives' pay should be correlated with the profit level of the firm. This relationship is, in fact, not significant. However, Simon demonstrates that organization theory provides another set of criteria which are more relevant. If we consider the top executive to be at the head of a multi-leveled hierarchy of employees, then the number of levels should increase with the size of the organization. If the organization must compete on the open market for new members to be added at the lowest levels, then the base pay for all firms at these levels is virtually the same. The increases in pay upon advancing to a higher level must be large enough to be discriminated, yet not wasteful. Accordingly, one would expect an organization with a greater number of levels to reach higher pay scales than the others with which it competes and that the top executive's compensation should be highly correlated with size of organization. It appears that executives' salaries are based much more upon an internal perception of appropriate rewards for responsibility undertaken than the imputed marginal returns to the firm due to his services

Rationality and Administrative Decision Making

The foundations has now been laid for the central theme of the work. Simon calls this the "principle of bounded rationality" and elaborates it in the following manner:

> The capacity of the human mind for formulating and solving complex problems is very small compared with the size of the problems whose solution is required for objectively rational behavior in the real world—or even for a reasonable approximation to such objective rationality.

If this is correct then the definitions of rationality being developed in modern economics and statistics are unrealistic because they are unattainable. He quotes an earlier work of his own:

[4] H. A. Simon. "The Compensation of Executives," *Sociometry*, 20, 32 (1957); D. R. Roberts, "A General Theory of Executive Compensation Based on Statistically Tested Proposition," *Quart. Journ. Econ.*, 20, 270 (1956).

> . . .if there were no limits to human rationality administrative theory would be barren. It would consist of the single precept: Always select that alternative, among those available, which will lead to the most complete achievement of your goals. The need for an administrative theory resides in the fact that there *are* practical limits to human rationality, and that these limits are not static, but depend upon the organizational environment in which the individual's decision takes place.

Therefore, if we wish to predict everyday organizational behavior, we must assume that there is a steeply rising cost to the computation of optima and that this cost becomes an important criterion in the making of choices themselves. Simon is highly critical of the myth of omniscient man and finds himself, in this respect, almost alone among the mathematical model makers.

Since he does not state the case for the opposition, I shall put together a short brief for them here. It is not very interesting for a facile brain to speculate upon what the ordinary mind might be capable of—this task is perhaps best done through the development of psychological measurements at an empirical level. What is really interesting is the uncovering of some new level of sophistication the human mind can achieve. A negligible expense can usefully be assigned to computational effort because in the long run such costs will be trivial as compared to the present. In the future, they feel, the most crucial decisions will not be bounded by the computational powers of the decision maker but by the instrumentation he is capable of employing. Their judgment regarding what is worth doing is based largely upon the shared esthetic values of the fraternity, but there is some justification for this esthetic in the technological trends that we can discern. Information handling *is* becoming cheaper.

A short side remark must be introduced here to take care of a special direction that model making may go which is represented by only one paper in this series. During the last War servomechanism theory was rapidly developed and applied to the control of mechanical and electrical systems, including some that were operated by humans. It was a very natural step then to extend the concepts of stability to systems containing many people and many machines, such as factories, transport systems, and even economic systems. An example of this mode of thinking so preliminary in its coverage that it stops short of the use of the Nyquist rule, is provided in an application to the control of production by the firm. At the time this piece was first available as a discussion paper (circa 1949) it was a most unusual and illuminating effort. Since then theoretical contributors in engineering have tried to unravel Keynes, mainly with the aid of servomechanism analogs,[5] while engineers responsible for production and supply systems have worked out many ingenious variations upon lesser themes. In a way it seems to be a matter of only a few years lag in development which kept this direction from being the principal foundation for econometrics. If that were so, a wide range of analog computers would very likely have been built for dealing with economic variables. (It will be noted that in Simon's models for social processes, which were taken up earlier, the relationships were set up as differential equations which are eminently suited to this mode of analysis.) But as things stand the field has familiarized itself with symbolic logic, matrix algebra, statistical decision theory, and digital computers, so that these will be employed for formalization wherever there is any choice. This observation is meant to be merely an insight into the bases for the style of contemporary model making in the social sciences.

The significant contribution to the simplification of rational decisions has been the formalization of a new criterion for choice. Simon noted that people and animals seldom, if ever, consciously optimized choices which were open to them in the manner ascribed to them by economists and statisticians.[6] Most people are reluctant to carry out

[5] Arnold Tustin, *The Mechanism of Economic Systems*, Harvard University Press, Cambridge, 1954.

[6] The classical framework for rationality of decision has been ably summarized by Sidney Schoeffler in his book *The Failures of Economics*, Harvard University Press, Cambridge, 1955.

mental computations, but even those that do, such as scientists and speculators, usually restrict it to a few phases of their daily activities. Simon looked into the literature on chess playing and into psychology for hints as to how people economize upon computation.

He contends that an organized system starts with an aspiration, based upon its prior history, which is "good enough" to get by. This is the degree of satisfaction with respect to a given end (say food or drink) which has worked out satisfactorily in the past. It is matched with a price per unit of commodity that is normally paid. In the absence of experience it may start with a level chosen at random. If the environment presents choices which are more expensive on a price per unit basis than this threshold they are likely to be declined. After a few such experiences however, the person is willing to raise the price he is willing to pay in order to establish a reasonable chance of achieving that end. Thus by raising (and lowering) the threshold of expectations according to the sequence of choices that are presented, a person can use very primitive computations and do well enough to get by.

Because this criterion for choice requires (*a*) information storage of very small dimensions, (*b*) equipment for making simple comparisons, and (*c*) a means for raising and lowering thresholds in response to need, it is likely to be the first criterion developed in the course of the evolution of organization. It is likely to be used wherever the decision is not crucial or where the data are not very trustworthy. He has applied a neat, economical term for this mode of choice—"satisfice." This approach when applied to two or more goals is still simple. Under many circumstances it does not even seem necessary to construct a priority scheme. It embodies the logic of Grey Walter's mechanical turtle.[7]

The beauty of this primitive criterion is that it is possible to build up a series of learned responses based upon it and even a logic machine which learns how to prove more and more complex theorems, or to play chess, or many similar tasks. His primary point is that, by simple additive rules, primitive rationality can meet relatively complex situations and deal with them adequately.

[7] W. Grey Walter, *The Living Brain*, Norton, New York, 1953.

The Measurement of Subjective Probabilities

Another most impressive attack on the nature of rationality in human decisions appeared more or less simultaneously with Simon's volume. The authors are a group of statisticians, philosophers, and psychologists who worked on the Stanford Value Theory Project.[8] They set up a unique framework for measuring the comparative value to an individual of various alternate choices (the economist's concept of utility) at the same time that they assessed the individual's judgment of odds. This has been a classic problem and one which, until only a few years ago, seemed to have no practical solution. Economists had been asserting for almost a century that consumers maximized utility—without ever devising a method for measuring utility. About the time that they were ready to look for a substitute, because utility did not seem to be an operational concept, some proposals of Friedman and Savage, which were taken up shortly thereafter by Mosteller and Nogee, introduced a means for measuring some features of the concept—particularly the utility of money—by experimenting with Harvard undergraduates and National Guardsmen of about the same age. They had done so by making one major assumption—that these young men attributed subjectively the same odds to the turning up of some class of numbers on dice as would be deduced from other, more scientific considerations. There was experimental evidence already available which seemed to show that people preferred risks at some odds against all others—this was particularly true of wagers at even odds (because the chances of making an error were least, this result is to be expected).

In the Stanford experiments on the utility of money the undergraduates in search of

[8] D. Davidson, P. Suppes, and S. Siegel, *Decision Making: An Experimental Approach*, Stanford University Press, Stanford, California, 1957.

part-time employment were asked to gamble with their prospective earnings from two hours of work. They were faced with two pairs of choices, A and B, and a die with four nonsense syllables on it (such as ZEJ, WUH, QUG, etc. previously shown by psychologists to be most immune to a "lucky number" bias). If A was chosen, the die would determine whether payoff A_1 or payoff A_2 would result. The experimenter would modify the payoffs until the subject was indifferent as to whether he chose A or B. Then he would explore this zone of indifference. They have a neat approach for translating the subjective feelings about equivalence of value into an equal interval scale. From this scale they have been able to get at the subjective probability that each of the individuals assigned to the upturn of a given face of the die.

As might be expected there was a wide difference among individuals with regard to their valuation of money. But more surprising—for this was a completely new result—most of them estimated the probability subjectively of a given face at about .20 instead of the apparently obvious .25 (they were assured that it was a true die before the experiment began). The interesting feature that came out was that these Stanford undergraduates were not consistent suckers. None of them were always over-conservative or extravagant. It is just that incremental amounts of money, even in these small sums, have different values for them—some much preferred a larger sum while others tried to make sure they held onto their original two dollars. Besides that, almost all of them proceeded by "feel' rather than compute on an actuarial basis the most profitable choice. As a consequence they seemed to infer that a moderately rare event (one in four) was rarer than objective calculations would make it.

Other experiments on the measurement of utility were carried out in an area where money is relatively meaningless—music appreciation. Overlapping collections of relatively uncommon classical records were to be compared with each other with the idea that the participants could keep a recording they liked better than some of the others, as determined by the upturn of ZOJ on the face of their special die. It was found that the equal interval numerical scale obtained predicted choices consistently better than a mere ranking of the preferences. From this experiment the authors launched into a mathematical analysis of the assessment of incomparable outcomes. The trick is to find a chain of comparisons (paired choices about which the person is indifferent) so that eventually a series of comparable outcomes is obtained. By such means, for instance, a chess player may equate the loss of a knight and a pawn with the loss of a rook even though they have quite different properties in the endgame. The analysis ends with a suitable climax—the formal axiomatization of the theorem which holds that it is possible to construct an equal interval numerical utility function together with a non-additive numerical subjective probability function (remember, the sum of all *subjective* probabilities does not add up to unity). Their little book will be a classic for many decades to come.

Exploring with a Game

These experiments, combined with Simon's model for a simple, locomoting organized system capable of doing no more than "satisficing," fascinated me immensely. In order to provide the beginnings of complexity Simon's hypothetical organism was endowed with two needs (say food and water) which required satisfaction. It could explore for these commodities, or it could rest. Simon was able to construct a relationship between the structure of the environment (i.e., the availability of food and water) and storage capacity which would also permit survival. If the food and water were distributed at random in the environment then death was highly probable within a short period of time, but if a few clues were provided as to its location then only a small amount of memory would be needed to very greatly improve the odds on survival. To carry this line of thought forward as a mathematical model yielding real solutions seemed to be far beyond our present capacities, especially if some of the food was prey

that locomoted on its own and there were other threats of extinction in the environment beyond those of starvation and thirst.

It occurred to me then that the possibilities in this "game against Nature" were so boundless that one might economically observe the evolution of utilities and subjective probabilities, as well as criteria for choice, with a game that could be played by both children and adults, and it might even amuse game theorists. No known parlor game existed in a form that could be easily adapted, so last winter a new one was designed and presented to the children for Christmas.

WILDLIFE is played on a chessboard where the respective spaces are so marked that they offer visibly varying odds and payoffs. White spaces yield "water" (white chips) and black spaces yield "food" (blue chips). One's animal is launched onto the edge of the "island" with six blue chips and six white chips. Each space locomoted costs one blue chip and one white chip (the possibilities for movement are the same as for the queen in chess, but in practice it is found that the animals amble about one space at a time). At any time that the animal's supply of either food or water disappears it dies. The odds are mediated by the tossing of a die.

At this level of complexity four- to six-year-old children quickly catch the idea and, except for occasional negligence, manage to survive by using the "satisfice" criterion. For the half hour or so at a time that they are content to play they remain quite content to "get fat." For those above six years a new source of interest had to be built into the game. Animals whose stocks of white and blue chips both exceeded twelve, could bring into the world a "baby" animal endowed with six of each color. Once the babies got fat enough they too could have babies, so that a player might in time be manipulating as many as a half a dozen animals of the same species. This possibility could maintain interest for more than an hour in seven- to ten-year-old children.

In order to introduce ecological reality a scheme was developed for a "battle to the death" where the odds of surviving were a function of superiority in "fatness." Such a battle required conscious aggression on the part of one child. This was resisted by the children, even though the animals were carefully chosen for low empathy (crabs, owls, mice, skunks, etc.). I have yet to get some good poker players into the game where each has bought chips from the bank ("Nature's reserve") and have no intention of losing that cash. Then perhaps some elements of calculated "high strategy" might be introduced.

Out of this observation of children at play, I have come to one conclusion. The routine decisions seem to be taken care of by some very simple criterion which could easily be "satisficing." But problems to which attention is directed (these are the "interesting" problems) tend to be treated in a more calculating manner.

Quastler and his Bio-Systems Group at the University of Illinois have completed some studies which provide a few clues as to the maximum dimensions of the problem which might be solved in the context of a game like this.[9] He showed that persons could pick up as much as 10–12 bits at a time of information under ideal conditions *if this information was distributed in seven (plus or minus two) categories.* Almost as much more information might be added from peripheral vision and from memory. In order to achieve this capacity the categories had to be more or less equally filled. Quastler felt that at peak many people might be able to combine something like the following:

3 different classes of input (averaging 5–6 alternatives apiece)
3 different classes of output (averaging 5–6 alternatives apiece)
2 decision rules (averaging 2–3 alternatives)

Up to 20 bits may be stored for a very short term (less than a tenth of a second) for purposes of decision. One deduces that, if a decision is possible, the system bursts like

[9] Henry Quastler, "Studies of Human Channel Capacity," and "The Informational Limitations of Decision-making," Report R-71, Control Systems Laboratory, University of Illinois, 1956.

a bubble and the registers are cleared within a second for a new decision. According to my own estimate it would take six categories (logons) and about six bits of information at such a conjunction to "satisfice." By ordering a series of small-scale decisions of this sort and storing the result, then starting from another direction and doing the same thing five or six times, and finally finding a unique solution for these stored results, one can in time solve highly complex problems. However, as soon as an important amount of experience accumulates, it appears that single decisions in humans tend to become more elaborate and sophisticated reaching the 20-bit levels suggested by Quastler's experiments. This is almost always the case for deliberated decisions within larger organizations.

For the next few years it appears that the modelmakers who are reconstructing the properties of man will be mapping the *heuristics* that humans have developed to meet various situations which demand choice in the midst of complexities. A heuristic is a simplifying device, or strategy, which enhances the likelihood that a good choice will be made. Many of these heuristics may be translated so that they can be employed by computers and, with a bit of experiment, the costs and benefits associated with each of them can be measured. A heuristic for finding heuristics is a game which resembles some aspect of life. It is only after this exploration has gone a long way that we shall see computing machines coded with hierarchies of strategies that approach the sophistication of the experienced scholar—which was stated as one of the more practical objectives of organization theory at the beginning of this discussion.

Explorations in the Realm of Organization Theory: II. The Organization of Space

Regional science[10] focuses upon problems for which spatial dimensions and spatial interactions are central. It is growing up in the interstice between economics, geography, political science, sociology, and anthropology. The theme which is of greatest interest is the *organized* quality of space on this planet. The parallels with organisms are frequently drawn upon in definitions of concept and discussions. One difficulty is admitted however. There are measurements for the organization of space at the "static" or gradually adaptive level, but dynamic models which would describe the *behavior* of regions are yet to be developed.

Still more interesting are the discussions of structure within a region as well as interregional relationships. The body of observations in a region may be sliced so as to obtain convenient subregions. It might also be dissected into fragments which reveal its economy, its political administration, or its socio-cultural organization. One investigator may use atomistic units such as families, individuals, transactions, or communications, another may use segments of varying scale such as institutions or communities.

The greatest sophistication has been achieved in the analysis of input-output models for the economy because in modern Western societies production data are regularly assembled under governmental auspices and they are by now reasonably complete. A classification of industries into such categories as fertilizer, plastics, synthetic fibers, explosives, etc., can be made so that the assignments of any unit activity to a category are not too ambiguous. One can describe the structure of a region in terms of the inputs and outputs of each of the industrial categories. It has been found however, that this description contributes very little that is useful until one takes into account substitutability of inputs, the change in ratio according to the size of operations, the innovations in technology, etc. Then it is possible to make some predictions concerning the economics of the location of new industrial activities and the effects of interregional competition upon structure.

Of course, for any single purpose, whether for electric power distribution or wild life

[10] G. A. P. Carrothers, (Ed.). *Regional Science: Papers and Proceedings*. Vol. 2. Cambridge, Mass.: Regional Science Assoc. (Wharton School, Univ. of Penna.), 1956.

preservation, regions can be identified as soon as the mapping of the activity has been completed. What is more interesting however is to find a class of purposes for which the same boundaries are set. Then investigations relative to one purpose will generate hypotheses concerning the spatial structure of other purposes. For some kinds of regions, such as that denoted by the metropolitan area, a vast number of interrelations can already be plotted although, due to the fragmentation of political decisions by a network of obsolete boundaries, many of these relationships cannot be optimized through planning and administration. The semi-permeable nature of political boundaries, and their extreme inflexibility, combine to create discontinuities in gradients and hinder comprehensive reorganization.

A general discussion of these matters was taken up by Walter Isard and Morris Garnsey and were continued in the papers concerning problems of planning in the United States. It is worthwhile taking up more briefly some of the other interesting contributions in the collection.

Harvey Perloff explored the relationship between per capita income and industrial structure in the United States and showed that the variation in income per worker from one industry classification to another was so great that the hypothesis advanced by Colin Clark (as per capita income increases an increasing proportion of the labor force will be employed in service type industries) hides the real changes that occur in the course of economic improvement and suggests new and more elaborate studies leading to the measurement of the degree of economic development of a region. Don Bogue has made an estimate of the amount of agricultural land that may be taken out of cultivation as a result of the spread of cities over the next twenty years. Despite rapid growth, his figures suggest that the problem is hardly serious over this period.

An abstract approach to defining the properties of the binding energy over space between individuals in a human population was reviewed by Theodore Anderson. Some applications of this form of model (which, because of its similarities to those used in astrophysics, is called a "gravity model") for the analysis of market potentials in Florida were described by Edgar S. Dunn. By such means the optimum location for a population-dependent operation (e.g., shopping center or pulp mill) can now be established with greater precision. Some disturbing exceptions to these generalizations as they apply to rural areas in the Pacific Northwest were introduced by Wm. L. Garrison.

Britton Harris discussed more rigorous procedures for projecting industrial growth of a region in the United States economy which are mainly based upon the availability of data for input-output and linear programming. Donald Foley analyzed the decisions regarding the location of administrative offices and found three important reasons for moving away from downtown San Francisco. This included (*a*) the decision to create a new plant or a warehouse and relocate the office to that site, (*b*) splitting off of certain functions from headquarters which were essential neither to the executive nor to sales, (*c*) the decision to expand to a size greater than that which the present headquarters would tolerate, followed by the choice of an outlying site which offered flexibility for this expansion. Decentralization has been found to be a relatively meaningless concept for these movements of administrative function once a large body of data had been collected.

There are included in addition some valuable discussions of these papers which enable the outside observer to get another slant upon the subject matter of the papers. Regional science is still too young a subject for the discussants to have become formal so that these short contributions tend to be more revealing than their equivalents in the symposia held by the established disciplines.

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ABSTRACTS OF CURRENT LITERATURE WITH INTERDISCIPLINARY IMPLICATIONS

Prepared by Members of the Staff of the Mental Health Research Institute

BELANGER, DAVID. **"Gradients" musculaires et processus mentaux superieurs.** *Canad. J. Psychol.*, 1957, 11, 113–122.

In this study subjects were required to perform perceptual-motor tasks of varying difficulty while changes in the muscular tension of the forearm were recorded electromyographically.

In the first experiment the subjects (eight males, six females, age range 18–32, right-handed) were presented with twenty series of six circles of varying sizes projected on a screen. The subjects' task consisted of designating the largest circle by number. Concurrently subject had to press a button with his left or right index finger according to instructions. These series were repeated at intervals of five, three, and two seconds between projections. Muscle potentials were recorded by means of surface electrodes placed on the flexors and extensors of the forearm. All subjects exhibited a gradual increase of tension within a series. This increase was present in the passive as well as the active arms and appears to be linear in the second case ($p < .01$). The gradients are most marked, however, in the extensors.

The second experiment was conducted to find out whether these gradients were attributable to the repeated finger-pressure movements rather than to the difficulty of the task. The experimental setting was the same as above, but this time subjects had to press a button when they perceived clicks which were presented at two-second intervals. Under these conditions, there is no evidence of either an increase or a decrease in tension.

The findings are in accord with Hebb's motivational theory. The author concludes that gradients of muscular tension observed during the performance of certain tasks are directly related to the higher mental processes involved in this task. They may thus serve as valuable indices of the efficient functioning of these processes. (*SK*)

CHASSAN, J. B. **On the unreliability of reliability and some other consequences of the assumption of probabilistic patient-states.** *Psychiatry*, 1957, 20, 163–171.

The extreme variability of psychiatric phenomena renders unreliable the usual criteria of data reliability. Thus the usual models of experimental design which work well for agriculture and industrial studies are inadequate to the demands of psychiatric and psychologic studies. The author recommends the "development and use of data systems consisting of large numbers of repetitions of observation within a framework of probabilistic or stochastic definitions of patient-states," which he regards as "a necessary complementation and refinement of relevant end-point data, such as diagnosis, age, sex, and other background variables which help to establish broad initial-state homogeneities, at the beginning of any study." (*NR*)

COHEN, SANFORD, I., SILVERMAN, ALBERT, J., AND ZUIDEMA, GEORGE. **Physiologic stress response evaluation by focused interviewing.** *A.M.A. Arch. Neurol. Psychiat.* 1956, 76, 670–674.

The authors investigate the relationship of emotional factors to tolerance to g forces (generated when an organism is subjected to acceleration). On the basis of previous research it is believed that hypertensive and hypotensive responses are associated with the direction of aggressive impulses, the former primarily with outward directed impulses, as in anger, the latter with inward directed ones, as in anxiety. Since the

physiological correlates of hypertension tend to counteract the centrifugal effect of acceleration and the hypotensive ones, on the contrary, to aggravate them, the study was undertaken to determine whether the *g*-tolerance can be predicted from the evaluation of the subject's attitudes in focused interviews immediately following centrifuging. The interviewer's estimates of each subject's blackout level on the basis of the interview were found to be highly accurate. The blackout levels having been divided into five classes, five of six subjects were correctly placed in their class of tolerance, while one subject was placed in a class immediately above his actual level.

The authors point out that physiologic patterns "may not correlate as closely with personality structure," but hold out hope that the affective state a at given moment "is closely related to neurophysiologic events in the limbic structures." (*AR*)

FRISHKOPF, L. S. **A probability approach to certain neuroelectric phenomena.** Massachusetts Institute of Technology, Research Laboratory of Electronics, Technical Report 307, March 1, 1956.

Experiments described in this paper belong to an extensive series pursued over the last several years at the Massachusetts Institute of Technology in connection with recording the neural responses to certain auditory stimulation of anesthetized cats. Characteristically the stimuli are "clicks" produced by brief rectangular pulse currents of 10^{-4} seconds duration. The responses recorded are from a location a few millimeters above the round window of the inner ear. The choice of location is governed by the attempt to eliminate the microphonic component of the response, leaving presumably the neural components to be recorded.

The clicks are varied over a wide range of amplitude (about 90–100 db). The response is recorded as a time course of change in potential, assumed to be a result of summated volleys of firing neurons. The most pronounced characteristics of the response record are an initial rise from the base line followed by a fall below the base line, followed by a secondary rise. A number of parameters therefore might be measured. The author chooses the height of the first peak, the area under the first peak, and the maximum potential difference as his intensity parameters. All of these, averaged for the several occurrences of the same click intensity, turn out to be approximately the same functions of that intensity, characteristic for each animal but differing markedly from animal to animal.

The author seeks to unify the experimental results by means of a theoretical model based on certain statistical properties of the "neural populations." He defines a "neural unit" abstractly in terms of its idealized properties. These are the usual ones—the all-or-nothing response and a threshold. This threshold is supposed to fluctuate in time so that its value is a random variable. No specific assumption is made about the nature of the distribution function of this random variable, an advantage from the point of view of generality of the model, that is, the avoidance of *ad hoc* assumptions.

Next the author defines a "population" of neural units as an aggregate with identical parameters, including the threshold probability distribution. The neural units are also supposed to be pair-wise statistically independent. These assumptions lead to the following expression for the average amplitude of response $\bar{A}(S)$ as a function of the stimulus S in a *single* population. If $D(s)$ is the probability frequency distribution of threshold values, $p(s) = \int_{-\infty}^{s} D(s)ds$, N the number of neural units in the population and r the amplitude of the individual response, we have

$$\bar{A}(S) = rNp(S).$$

The formula is immediately generalizable to k populations, each with its characteristic number of neural units, response amplitude and distribution function, namely,

$$\bar{A}(S) = \sum_{i=1}^{k} r_i N_i p_i(S).$$

The next theoretical step is the derivation of the *variance* of the response amplitude as a function of the stimulus. This turns out to be

$$\sigma_A^2(S) = \frac{(A_{max})^2}{N}\left[\frac{\bar{A}(S)}{A_{max}}\left(1 - \frac{\bar{A}(S)}{A_{max}}\right)\right].$$

Since $\bar{A}(S)$ and A_{max} are experimentally determined, $\sigma_A^2(S)$ is also determined within a multiplicative constant N (population size). Statistical independence now gives the variance associated with several populations as simply the sum of the respective variances. (The variance due to external sources is lumped into another constant.)

The data now allow the construction of a graph of the intensity of response and of the variance (or standard deviation) as functions of stimulus intensity and thus to compare the theory with experiment.

Frishkopf first postulates the existence of a single population of neural units and finds this hypothesis justified by the data in the range from −100 db to −60 db. The model also enables him to estimate this population as consisting of 150–400 units. (It should be emphasized that this hypothetical "unit" is not identical with the neuron but is more likely to be composite entity, the exact structure of which is unknown but which turns out to be a useful theoretical construct.) Over the remaining range, however, (−60 db to 0 db), the single population hypothesis does not account for the data within the framework of the model.

The results seem to point to a picture which is already suggested by an inspection of the graphs of response intensity. These graphs all appear to show a two-stage growth, the first stage ranging to −60 db, the second from then on. The foregoing theoretical analysis supports an interpretation of this crude observation in terms of a more definitive model involving a single homogeneous population of sensitive units and another probably heterogeneous pool of relatively insensitive units. This conjecture seems to be further supported by another set of experiments, the so-called "masking studies," concerned with the effect of background noise upon the amplitude of the responses. In these experiments, the intensity of responses to clicks is depressed throughout. The depression is interpreted by the refractoriness of neural units, presumably fired by the noise but whose responses, not being synchronized, are not detected. Thus the same model is applicable under another set of conditions and is further corroborated.

In the last section of the paper the author discusses the discrepancies between his single population model and the data. These are small and systematic in the low intensity range and large and systematic in the high intensity range, necessitating the rejection of the model in that range. However, the systematic nature of the discrepancies throughout leads the author to a number of interesting conjectures which may lead to extension and refinements of this approach. (*AR*)

GOLD, DAVID. **On description of differential association.** *Amer. sociol. Rev.*, 1957, 22, 448–450.

The author provides a formal method by which a differential association between a given independent variable and a dependent variable can be tested. Reference is made to the fact that in sociological research inferences are frequently made to the effect that one category of a given independent variable is "more important" than the other categories with respect to a given dependent variable. For example, in studies of voting behavior, it has been explicitly suggested that Catholic affiliation is more important than Protestant affiliation for partisan voting. That is, one category of the independent variable, religious affiliation, is said to be more important than another in relation to the dependent variable, partisan voting. It is this kind of motion which the author labels differential, as opposed to total, association. It is pointed out that since there is no precise focus upon differential association as such in sociological research, this is a relationship that has often been overlooked.

What then is the method by which a differential association may be tested or, stated concretely, what does it mean to say, for example, that Catholic affiliation is more important than Protestant affiliation in

voting? The author indicates the following technique for answering this question. A partial association of the above kind may be detected by working with two fourfold tables, one for Catholics and one for Protestants, each cross-tabulating, say psychological identification (i.e., "the church is important to me" versus "the church is unimportant to me") and vote (i.e., Republican versus Democrat). An independent variable (religious affiliation) is thus held constant. The focus is here upon the partial associations between a second independent variable (psychological identification with the church) and the dependent variable (vote). Suppose it is observed that the relationship between psychological identification and voting is greater in the one category (Catholic) than in the other (Protestant). On this basis it may be inferred that Catholic affiliation is more important than Protestant affiliation in voting because strong versus weak identification with the church makes a difference in voting behavior for Catholics but not for Protestants. The author points out that the second independent variable (psychological identification with the church in the above example) may also be in the nature of, say, a socioeconomic variable like occupation. Thus if it were found that membership in particular professional groups makes a difference for Protestants but not for Catholics, a conclusion concerning the greater importance of Catholic affiliation in voting would again be warranted. The author states his position formally as follows. Suppose an independent variable (x) and a dependent variable (y), and the hypothesis that x is differentially associated with y. In order to test the hypothesis, a second independent variable (w), relevant to y, is introduced. The partial association of w and y in each category of x is then observed. The given substantitive considerations involving the nature of each of the variables (w, x, and y) in relation to each other indicate the inference of "importance" that can be made from the relative sizes of the partial associations.

The author concludes with a warning not to make unwarranted predictions from analyses of differential associations. He points out that the meaning of "more importance" is founded on the difference between degrees of relationship that exist in a pair of contingency tables taken as a whole and not on the difference between *particular* frequencies in the contingency tables. (*JWG*)

LANDAHL, HERBERT D. **Population growth under the influence of random dispersal.** *Bull. math. Biophysics*, 1957, 19, 171–86.

The author had previously developed an approximation method for the solution of diffusion and related problems (*Bull. math. Biophysics*, 1953, 14, 49). Simple linear expressions are substituted for the correct solution; the parameters in these linear approximations are determined from the differential equation to be solved as well as from the boundary conditions, and the initial choice is made according to physically plausible assumptions. For example, in the simplest case of one-dimensional diffusion and no sources, the linear approximation is taken to be $c(x, t) = c_0 (1 - x/r)$ for $0 \leq x \leq r$, $c(x, t) = 0$ for $x > r$, where D is the diffusion coefficient and c_0 is the initial concentration at $x \leq 0$ ($c = 0$ initially for $x > 0$); r, a function of t, is a length beyond which the concentration will be zero. It turns out that $r = 2\sqrt{Dt}$.

The same method is now applied to the differential equation $(\partial n/\partial t) = \alpha n - \beta n^2 + D\nabla^2 n$, n being a population density. D denotes again the diffusion coefficient (assumed constant); the positive constants α and β are, respectively, the coefficients of birth and death rate. If the term containing D were absent we would have a simple growth of population according to the logistic equation. Superimposed on this is a diffusion effect, on "spreading out" of the population.

The approximate solution, with numerical tables and graphs when necessary, is given for two-dimensional cases with central symmetry. Other examples studied involve one-dimensional diffusion with absorbing and reflecting barriers, and also "refraction", i.e., the transition from a region characterized by constants α', β', D' into another

region having different constants α'', β'', D''. Transient as well as steady-state solutions are discussed. (*ET*)

LANZETTA, J. T., AND ROBY, T. B. **Group performance as a function of work-distribution patterns and task load.** *Sociometry*, 1956, 19, 95–105.

After reference to prior research on the relation between group structure and group performance by Bavelas, Christie, Leavitt, Hurwitz, Lippitt, and Carter, the authors indicate the absence of research in which particular distributions of task responsibilities are related to group performance, and point out that this is a problem area which precedes considerations of leadership and communication structure.

They aim to start empirical work in this area and in the present paper report on a study which investigates the relationship between two methods of work distribution and group performance under two task load conditions.

The task was modeled after that of an Air Defense Command Aircraft Control and Warning Center. Subjects were faced with a 6′ x 5′ board with a 50 by 40 grid identified by coordinates. Three discrete target areas were located near the center of the board. Model planes with small attached magnets were used to provide an "enemy" and "friendly" air force. Three different colors identified enemy planes, unidentified planes (which could be friendly aircraft in the area), and friendly interceptors, respectively. Interceptor planes were stationed at three airports close to the three target areas. The tasks confronting the group were: (1) to monitor the position report of all planes (other than friendly interceptors) which came into the subjects over a tape recorder and to make the necessary plane moves on the intercept board; (2) to identify aircraft as friendly or enemy from the color of the planes as well as from an input concerning "flight plans" of friendly planes, and also to keep track of the fuel status of interceptor aircraft on the basis of knowledge concerning time spent "in the air"; (3) to make decisions concerning deployment of the friendly interceptor force.

The experiment called for the creation of two distributions of task responsibilities (i.e., structures) under two task load conditions. One structure was based on a functional classification of activities (vertical structure) and the second on a subtask discrimination (horizontal structure). Thus under the condition of vertical structure one group member became the "observer," another the "calculator," and still another the "decision maker" in conformance with the 3 main functional divisions of the group task. Under the condition of horizontal structure each group member performed all three of the required functions, but was assigned responsibility for only one target area and one interceptor base. As to the task load conditions, two levels of task load were used. Under high-load condition 15 planes were employed (nine enemy bombers, six friendly planes for which flight plans were submitted), whereas under the low-load condition 10 planes were employed (six enemy bombers, four friendly planes with flight plans).

The design was a modified latin square. A total performance score was computed on the basis of an arbitrary assignment of points for downing enemy planes, and penalties for downed friendly planes.

The findings concerning the interaction between structure and task load in relation to performance are insignificant but nevertheless worth mentioning. It was found that horizontal structure was superior under low-load conditions while the vertical structure was superior under high load conditions. Structure itself was not significantly related to performance. There is an insignificant tendency for the performance of the horizontal structure to be superior. Significant findings appeared from unexpected quarters. It was found that task load and the interaction between task load and session were significant. High-load conditions resulted in poorer performance. However, under high-load conditions performance improved with practice, whereas under low-load conditions the reverse was true.

In discussing the lack of significance of the findings in regard to the interaction between structure and task parameters, the authors

point out that the complexity of the tasks, resulting in large within-condition variance, combined with a lack of initial performance matching, may have decreased sensitivity to the experimental treatments. In view of the exploratory nature of the study, the authors are willing to do some interpretation of the data in regard to the interaction between structure and task parameters. It is reasoned that the most difficult aspect of the task was "tracking" and "plotting" the course of unidentified aircraft. The other activities, deployment of the "interceptors," identification of aircraft as friendly or enemy, and keeping account of interceptor fuel status, were less difficult though of equal importance for group performance. If, under high-load conditions, subjects "fixate" on the difficult functions, it may lead to neglect of certain other functions and to less effective performance in the horizontal structure condition where each subject has to perform every function. Fixation would not affect the performance of the vertical structure negatively. The authors conclude that any hypothesis suggested for further test must be based on the characteristics of the task utilized in the study. The findings regarding the interaction between task load and session are explained by assuming that the initially poorer performance under high-load conditions is responsible for a "set" in the subjects to improve performance, whereas the relative effectiveness of the subjects under low-load conditions leads to complacency. (*JWG*)

NASH, D. J., AND WOLFE, A. W. **The stranger in laboratory culture.** *Amer. sociol. Rev.*, 1957, 22, 400–405.

Using the methodology developed by Rose and Felton (*Amer. sociol. Rev.*, 1955, 20, 383–392.) to produce an experimental analogue of cultures in small groups, the authors investigated some aspects of the MARGINAL MAN. The procedure requires that individuals in a group examine a Rorschach card, write down the concepts seen, then explain them to the other members of the group. This process is repeated several times, each repetition being an EPOCH in that culture. After three such epochs, one individual of two concurrently running groups was switched, thus creating the STRANGER in the groups. After three more epochs, these same individuals were returned to their original groups, thus creating the RETURNEE. The groups were then run for another three epochs. The interest was in the quantitative changes of responses of the experimental individuals (stranger-returnee) in relation to those who remained in the same group throughout the experiment. Responses were categorized as INVENTIONS, BORROWING, or HABIT, as in the Rose and Felton study.

Results show a decline in inventions of strangers in the new group and an increase in inventions with return to their old group. The borrowing and habit responses did not change significantly. The discussion indicates that such behavior changes may be some as yet unexplicated aspect of perceptual defense. These results also contradict the usually held notion that marginal man is highly creative. Doubt is thrown on the significance of this finding by appeal to the differences between "real" cultures and these experimental groups. (*AH*)

SCHULTE, ARNHILT. **Transfer- und Transpositionsversuche mit monokular dressierten Fischen (Experiments on transfer and transposition with monocularly trained fishes).** *Zt. vergl. Physiol.*, 1957, 39, 432–76.

Crucian Carps (*Carassius vulgaris*) were trained to discriminate monocularly between two visual stimuli. Testing began only after the animals had given 90 or more correct responses in 100 trials performed over a period of four days. To achieve such a high degree of learning more than 800 trials were necessary in some cases. Several pairs of stimulus patterns were used. The number of animals which could be trained successfully and which, later, were able to perform the task with the untrained eye, ranged from 6 to 18 in various experiments.

First it was established that a high degree of interocular transfer exists for the simple discrimination task. The animals performed equally well, or nearly so, when choosing the correct signal with the trained or with the

untrained eye. In these fishes there is complete crossing over of nerve fibers at the optic chiasma.

Subsequently, the task was made more difficult by introducing relatively minor changes in the stimuli (transposition). If, e.g., the animal had been trained to distinguish a diamond-shaped figure from a rectangle, the experiment consisted in testing for discrimination between a "diamond" and a "rectangle" with rounded edges. Other variations included changes in size or in color contrast between the test stimuli and the training stimuli. Here it was found that transfer of discriminating ability from the trained to the untrained eye persisted as long as the additional complication due to transposition was not too great. That is, there were "simple" cases of transposition in which the animals discriminated equally well with either trained or untrained eye. At the other extreme, the animals were unable to solve the problem with either eye (even binocularly trained animals failing to distinguish the transposed patterns). But in experiments of "intermediate" difficulty the animals performed very well with the trained eye and failed when tested on the untrained eye (loss of transfer). In some cases completely different results were obtained from each individual animal.

An attempt was also made to train the fishes on one pattern with one eye and the opposite pattern (reversal of "correct" and "wrong" response) with the other eye, but these attempts failed. This is in contrast to the results obtained by Levine for pigeons, where there is no interocular transfer of visual discriminating habits acquired monocularly (*J. gen. Psychol.*, 1945, 67, 105).

Control experiments established the fact that binocularly trained animals reach the learning criterion more rapidly than those trained monocularly; only in exceptional cases did some individuals of the latter group learn as rapidly as those of the former group. The difference was particularly strong during the initial phases of training; the final stage (from 70 or 80% correct responses to criterion) required a long time even for binocular training. Similarly, in the transposition test it was found that on the whole binocularly trained and tested animals performed better than those trained monocularly (tested on the trained eye).

On the other hand, forgetting was not significantly, or not much, faster in the monocularly trained animals than in the binocular group. This was shown by performing the tests at intervals of 7, 14, 21, and 28 days after criterion was reached.

"Neuroses" (characterized by exaggerated reactions to external disturbances, general restlessness, and changes in body color) were frequently observed. Possible causes for such abnormal behavior are discussed in relation to the experimental condition. (*ET*)

WAGNER, R. **Biologische Regelung und Gewebsbildung (Biological self-regulation and tissue formation).** *Die Naturwissenschaften*, 1957, 44, 97-107.

In this survey article the author emphasizes first of all that self-regulation of important quantities or functions by means of feedback mechanisms must be regarded as a very general biological principle. In particular, cell division and cell proliferation may be considered from this point of view, namely as a self-regulation for the amount and exchange of metabolites. Applying this to the oxygen consumption inside cells the author concludes that cell division is the best, and perhaps the only possible way of eliminating all the difficulties which arise in furnishing the center of the cell with an adequate amount of O_2 as the cell dimensions increase. Thus, cell division would be primarily (though not exclusively) a regulatory mechanism for keeping the O_2 supply at a constant level. The actual complicated sequence of events by which the division occurs is not important for this argument; the main point is that increasing need and diminishing supply of O_2 in the central regions may act as a trigger for the whole process. The author shows that several seemingly unrelated biological phenomena can be understood by means of this general principle. (*ET*)

WILLIAMS, ROBIN M., JR. **Religion, value-orientations, and intergroup conflict.** *J. soc. Issues*, 1956, 12, 12–20.

Williams points out that although there is widespread interest in the increase in "religious conflict" in the United States, from the point of view of social science the phenomena involved are not well understood: "the sources and consequences of those human conflicts which are posed in terms of differences in religious beliefs, evaluations, rituals and modes of organized social action" are inadequately explored. In the hope of stimulating necessary thinking and research the significance of value-orientation in interreligious relations is pointed out and some specific differences in values (between groups), and the ways these differences may affect interreligious conflict and cooperation, are suggested. No codification of variables, hypotheses, or empirical findings is attempted.

Relations between racial groups, those between ethnic groups and those between religious groups are all called intergroup relations. The point is made that interreligious relations are not adequately explained in terms of variables discovered in other intergroup relations. Interreligious conflict occurs when prejudice, such an important factor in interracial and interethnic conflict, is absent, when members of both groups are "initially devoid of stereotypes, ignorance, standardized modes of scapegoating, or diffuse unfulfilled needs for aggression." It occurs because religious values, i.e., "those conceptions of the desirable that are 'ultimate' for people who are committed to them" (thus final and irrevocable), are by definition not subject to compromise or resolution. But since the "social absoluteness" of religious orientations is a matter of degree, focal empirical considerations are the conditions under which systems of religious values allow for toleration, compromise, insulation and partial incorporation, and the conditions under which systems of religious values result in conflict, domination, and persecution. These conditions will involve variables both from the social context and the relevant schemes of religious value-orientations. As Williams sees it, the problem should be stated as follows: "specifying the variables that help to account for varying prevalence and intensity of conflict as a function of variations in religious value-orientations."

The dual social role of religion is reviewed "as either a focus of consensus, solidarity, and integration or as a source of societal cleavage and implacable opposition." And working definitions are offered. Values are "consequentially important conceptions (standards) of desirability which influence behavior and to which conduct is referred for judgments of goodness, appropriateness, and the like." Religious values are those that have "some direct connection with a nonempirical, usually transcendent, view of life and the cosmos." The social unit concerned is made up of "members of a specific religious 'communion' which constitutes a supreme center of loyalties." Then Williams suggests some issues involved in interreligious conflict. 1. Strictly religious tension is distinguished from other conflict that occurs in the name of organized religion. 2. The different results of various levels of intensity of conviction on the part of group members are discussed. 3. The function of a core of common values running through otherwise different religious systems is described. 4. The extent to which religions make a religious-secular distinction, and what aspects of life they assign to each area are discussed from the point of view of implications for conflict.

A list of twenty specific divergences between religious groups is submitted as ones that will probably turn out to be among the most important in interreligious opposition.

The article closes with suggestions regarding further research and conceptualization. (*MJ*)

ABOUT THE AUTHORS

Robert P. Abelson ("Symbolic Psycho-Logic: A Model of Attitudinal Cognition") is now spending a year as a Fellow at the Center for Advanced Study in the Behavioral Sciences while on leave from his position as assistant professor of psychology in Yale University. He has a bachelor's and master's degree in mathematics from the Massachusetts Institute of Technology and a doctorate in psychology from Princeton University. He states that his interests have undergone a gradual transition from mathematical statistics per se through traditional psychometrics (while a Psychometric Fellow at the Educational Testing Service), and finally to the broad application of mathematics to psychology. For several years, and particularly during the course of work with Robert R. Bush and Ray Hyman on a Social Science Research Council monograph, *Mathematics for Psychologists*, he has felt that the existing mathematical applications to psychology often cry out for anchorage in psychological theory. He is, therefore, concerned with the development of better mathematical models in several social psychological contexts. During his year at the Center he plans to work on extensions and applications of the model of the present article.

Donald T. Campbell ("Common Fate, Similarity, and Other Indices of the Status of Aggregates of Persons as Social Entities") was educated at the University of California, where he took the Ph.D. degree in 1947. He is now associate professor of psychology at Northwestern University. In describing his interests and research he states that in addition to being a hard-headed data-collecting social psychologist he also has a more speculative interest in what he calls "the comparative psychology of knowledge processes." For example, he offers a graduate course at Northwestern which purports "to compare blind trial and error, perception, imitation, thought, and language as knowledge processes." The current paper, in drawing parallels between visual processes and possible modes of data analysis in social science, illustrates this interest.

John Marcell Davis ("The Transitivity of Preferences") is a student at Yale Medical School, having recently graduated *summa cum laude* in psychology from Princeton University. His paper is the result of work done in the psychometric laboratory at Princeton, where he also worked on projective testing, perception and personality, and personality correlates of aptitude. During the summer of 1956 he did anthropological field work in Jamaica, on the phenomena of spirit possession and trance states. Presently he is interested in the integration of basic medical and behavioral sciences with psychiatry, and his current projects include investigations of mental disease and social class, physiological and psychological responses to severe life stress, and behavioral effects of tranquilizers.

Milton J. Rosenberg ("Symbolic Psycho-Logic: A Model of Attitudinal Cognition") was born in New York City, received his academic training at Brooklyn College, University of Wisconsin, and University of Michigan where he took his Ph.D. in 1953, and is now assistant professor of psychology in Yale University. Most of his research is in the area of persuasion processes, with particular emphasis on cognitive structure, affective-cognitive interaction, and personality variables. In addition to his work on symbolic psycho-logic he has been engaged during the past two years in the study of the consequences for cognitive structure of the production of affective-reversal through hypnotic suggestion. The present article and the further research that he and Dr. Abelson are doing represent a fusion of their separate interests in the problems of mapping and modeling cognitive processes in terms of their structural properties.

Hans H. Strupp ("The psychotherapist's contribution to the treatment process") has recently been appointed associate professor of psychology in the departments of psychiatry and psychology at the University of North Carolina and director of psychological services, North Carolina Memorial Hospital. Born in Frankfurt am Main, Germany, he received his Ph.D. from George Washington University. In addition, he holds a Certificate in Applied Psychiatry for Psychologists from the Washington School of Psychiatry. He is keenly interested in research on problems of psychotherapy, including psychoanalysis, and is convinced that the scientific method has a great deal to contribute to our knowledge of this area. He states, "As one whose education and career were forcibly interrupted by the Nazi dictatorship, I am deeply grateful to this great wonderful country and its institutions for the opportunity of development it has provided."

CONCEPTS OF BIOLOGY

Edited by R. W. Gerard

University of Michigan

Assisted by Russell B. Stevens

The George Washington University

Based on a Conference Sponsored by
The Biology Council, Division of Biology and Agriculture
National Academy of Sciences–National Research Council

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BIOLOGY IN *BEHAVIORAL SCIENCE*

This special issue of *Behavioral Science* is devoted to publication of material that will subsequently appear in book form under the auspices of the National Academy of Sciences. Although the participants in the conference that this reports were primarily biologists, and the title of the volume is "Concepts of Biology," its inclusion in this Journal is based upon our conviction that this symposium is of interest to many besides professional biologists. Here is exhibited current thought by leaders in the field, largely in terms and at a level of discourse that is nontechnical but which represents the distillate of thought in this mature area of knowledge. Explicitly in many exchanges and implicitly in a great deal of the material is the thought that similar concepts, problems, considerations, and models, must apply to a greater or lesser extent "above and below" the sector of the universe cut out by biological science. This is particularly of interest in relation to the social science area which, with biology, constitutes behavioral science. This symposium is presented in the hope that many social as well as biological scientists will find fresh springs of inspiration for their own professional thinking.

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FOREWORD

The following conference report centers on the question of whether present-day biology is paying too little attention to its conceptual maturation and, if so, why. Scientific knowledge grows much like an organism: feeding on empirical facts, digesting them and assimilating them into its body substance, which thereby gains not just in bulk, but in differentiation and maturity as well. Concepts are the structural elements of that growing body of knowledge. Until distilled and ordered into concepts, data remain but a pile of incoherent informational items. Concepts in need of verification and consolidation guide the search for new data, and new data, in turn, act as tests—and failing validation, as modifiers—of existing concepts. Thus, concepts and data must try to stay abreast.

Then, why does contemporary biology show—or, at least, appear to show—a marked disproportion between sheer empiricism and conceptual development in disfavor of the latter? Is this a fact or an illusion? And if true, is this simply an expression of the relatively immature state of biology as an objective science; or is it perhaps, conversely, co-responsible for the prolongation of this state of immaturity? Has the momentum of scientific routine in teaching and research swept us so far away from the mainsprings of our science that we have lost sight of the essentially intellectual nature of the scientific process? And consequently, are we drifting into a habit not only of dispensing with conceptual ordering, but almost of questioning the respectability of conceptual effort? These questions must be of serious concern to all of those who take the furtherance of the life sciences to heart. Therefore, to bring the issue into the open, the Biology Council, a deliberative body of the Division of Biology and Agriculture of the National Academy of Sciences–National Research Council, convoked a priming conference of representatives from a wide variety of biological disciplines, not for the purpose of trying their hand at "designing concepts" (which would have been not only presumptuous, but unrealistic), but rather to examine whether or not the purported deficiency actually existed, and if so, to what to ascribe it and how to meet it.

Dr. Ralph W. Gerard, a member of the Biology Council, undertook to organize and conduct the conference, which was held at Lee, Massachusetts, from October 12 to 14, 1955. He was assisted in his task by Dr. David R. Goddard, another Council member, and particularly by Dr. Russell B. Stevens, Executive Secretary of the Council. The latter also assumed the heavy duty of editing and integrating the transcript of the discussion. As will be seen from the text of the edited transcript, he has discharged this task most creditably by the split device of presenting lively and crucial parts of the discussions verbatim, while reporting less pregnant portions in condensed form. It seemed much more important to reflect the trends of thought pervading the conference than to pretend that any hard-and-fast conclusions have been reached. In fact, if this conference were to remain an isolated event, it would have missed its broader purpose. Only as starting point for a continuing process of intensified preoccupation with biological concepts can it have full effect. For having set a useful pattern for the future, we are deeply indebted to all participants and especially the Chairman, Ralph W. Gerard, who by expert steerage kept the discussion well focused on objectives sometimes only dimly perceptible through the fog that still envelops "biological concepts."

PAUL WEISS
Chairman, Biology Council

PREFACE

The Biology Council, conceived and guided by Dr. Paul Weiss, undertook a number of tasks concerned with the structure and setting of biology; with the supply of instruments and type collections and technical assistance for research, with the public image of this continent of science (perhaps suffering from balkanization), with attracting and then exciting the young by creative teaching, with exhibiting the organic structure of the body of biological knowledge, with other meta scientific aspects of our profession.

This monograph is an outcome of one of these tasks. Beginning with suggestions from the Council as a whole, and gathered by a small task force of Drs. Goddard, Mayr, Weiss, and myself, the preliminary efforts led to a working paper called "Concepts and Principles of Biology," which I submitted to the Council and which opens this monograph. Brief consideration of this paper convinced the Council that only the concentrated attention of a small group could contribute to further development; and the conference at Lee, Massachusetts was authorized. Dr. Stevens and Dr. Paulson, in consultation with Dr. Weiss and myself, arranged this meeting, took recordings of the discussion, and in other ways facilitated a happy outcome.

The talk continued over three days and much of two nights. The planned course was little followed, as the flood of ideas overflowed into wide territories; but an honest, exciting, imaginative, and occasionally ordered interchange took place. We all felt refreshed and educated by our experience: and more and more our refrain was that the process rather than the product was the greater good. If only students could first listen to and then participate in such "bull sessions," if teachers would lead their cohorts to the mental jousting fields of biology, what fine and productive enthusiasm would be kindled!

The free give and take of thought is only possible in limited groups, and any record is necessarily bleached; yet we hoped to communicate the contagion to some extent. A simple listing of conclusions was unthinkable; even an integrated summary of the problems and the pro and con considerations deserves a place more as a special article. Such an article, which originally appeared in *Science*, is reproduced as Appendix II. Rather, a tape recording of the animated discussion or, a less vivid alternate, a transcription of it could be made available; hopefully to serve to trigger off like adventures by student and teacher groups at many levels of biological sophistication. Since a full reproduction would be wasteful and impracticable, Dr. Stevens undertook to eliminate irrelevant material, condense relatively thin portions, and indicate transitional steps, while leaving large chunks of the record with only such minor editing for form and clarity as the speakers or he felt desirable. The bulk of this volume is thus the verbal spoor of a thirteen-minded epiorganism, as skillfully trimmed by Dr. Stevens. To him we are also indebted for the index, as we are to Dr. Spuhler for the selected reference list (Appendix III). We hope an edited tape, giving about thirty minutes of discussion focused on one topic, will be available for teaching purposes; certainly it will be if this volume proves sufficiently useful.

I cannot close without a word of thanks to fellow members of the Biology Council, including its intrepid chairman, for valuable seeding, and especially to participants in the Lee Conference for liberal fertilizing, of the ground covered. The growth has been luxurious; we trust some flowers bloom among the weeds.

R. W. GERARD

CONCEPTS AND PRINCIPLES OF BIOLOGY

Initial Working Paper

R. W. GERARD

An examination of the discipline, biology, in terms of its organization, methods, relations, and particularly its conceptualization, could serve many useful ends. Any success in improving the intellectual ordering of our subject would contribute to improved public relations, to the recruitment of more superior students, and to a better internal structure which would favor better teaching and research and in turn attract more students and support.

At the same time, it is clearly recognized that any effort by any organization to formalize an area of activity and knowledge carries the danger of imposing external categories upon varying individuals, categories which are neither effectively internalized nor willingly conformed to. What we can hope to do is offer aid and encouragement for each biologist to formulate his own conceptual scheme of biology; in fact, the main advantage of an effort along these lines, sponsored by such a body as the Biology Council, might well be the explicit sanction it carries that respectable professional biologists do not waste their time when some of it is devoted to such general problems and thinking. Too much detailed technological type of thinking is prevalent in science, having been fostered by pressures of administrative selection for positions, sources of financial support, and editorial policies on publication; so that it is important to emphasize that concepts are of value, of the greatest value, both in teaching and research, and that there is a vast difference between generalizations and generalities.

More particularly for professionals, such an effort should help reveal the state of our science and exhibit areas in which there is relative over- and under-emphasis in research and teaching, and might point up trends and needs for the future. An analysis of concepts and terminology, moreover, should help the recognition of phenomena which are genuinely common to many different areas of biology, to eliminate literary and vague terms which give a spurious sense of explanation or even of description, to eliminate complex and repetitive terminology, to particularize general and confusing terms which mean many different things to different people, and to encourage dimensional analysis and thinking. Dimensional analysis strongly favors the assigning of precise meanings to terms, encourages comparison of terms of like dimensions and enables one to come at the dimensionless core of concepts, and should encourage a type of scaling that reveals true relationships. Thus, when chemical quantities are expressed in mols rather than in grams innumerable insights emerge almost automatically; perhaps appropriate scaling of biological times in terms of life spans, expression of variation in terms of standard variance units, an emphasis on topological rather than geometric relationships, and the like would pay great dividends in general biological thinking. Finally, one might hope for an improvement in the characteristic thinking of a biologist, dealing with complex dynamic variables; and for improvement in interdisciplinary communication and understanding; with the results: that the different branches of biology would be brought more into consonance with one another, that freer criticism would flow between men in the different branches, that discussions would become more interpretive, and even that better publication outlets would emerge.

At the level of external relations, improvement might result in our relations to the public, to students, and to other scientists. Our own recognition of the great principles and uniformity of biology as a whole will

necessarily enhance our pride and confidence as biologists and our ability to emphasize its important contribution to society, its growing precision and power, and the consequent multiplication of opportunities for both employment and service. Moreover, an examination of the trend toward dynamic analysis and historical understanding, as contrasted to the more traditional static emphasis on description and classification, is bound to improve the public image of the subject and to increase the interest of the layman in such areas as dynamic human biology—interests which are potentially great but still largely unserved.

Comparable gains would be achieved vis à vis undergraduate and graduate students. The better students will be attracted into our subject, as against some other one, in proportion as the breadth of the generalizations and the depth of analyses supplement and in part supplant the dull or glittering collection of details. Such concept analysis might reveal the many vestigial regions of biology which are still taught, although the intellectual interest which brought them into being has long passed on to other areas. It is interesting, for example, that despite the extensive openings and high salaries available in anatomy departments there remain few takers—unless the old orientation of gross anatomy on organ systems has been supplanted or supplemented. Moreover, if teachers are encouraged by such a document to examine their subject matter it is likely to result in richer material being presented to students and to encourage teaching by the project type of approach, which favors the recognition and tackling of a problem by the student rather than the amassing of simple techniques or information. Finally, along these lines, the appropriate pretraining for various aspects of biology might be considerably clarified.

The relations of biology to other sciences should certainly become clearer, and those aspects in which biology is unique, whether absolutely or relatively, will become clear. Thus, although biology is demarcated from the physical sciences "below" and the mental and social sciences "above," it is perhaps not a linear intermediate but is displaced somewhat in the direction of engineering or of architecture. The biologist will lean even more heavily on the physical sciences without losing his distinctive flavor of thought and action. The biologist, being accustomed to working with complex systems and unprecise data, has proven himself able to cope with many practical problems that were too frustrating for the physicist or the mathematician to handle. He thinks in a different climate, in which such things as directiveness and adaptation play a disproportionately great role; he is aware, as are few physical scientists, of the necessity of including controls—because of the controllability of the multiple variables of the living system and its environment; he is concerned with systems that are rare in classical physics and chemistry, in which trigger effects dominate, in which there is a dissociation in time and quality between the physical change and the biological response, and in which the past of the system tremendously influences its future; and he deals with generalizations which need not be reduced to a physico-chemical basis, whether they can be or not, such as the gene as a biological entity, the taxonomic relations of organisms, the phenomena of individuality and specificity, of behavior of variation, of populations as entities, and the like.

In the other direction, there is far too little awareness of the contiguities as well as the demarcations from mental and social science. Psychologists, cultural anthropologists, and sociologists—even economists and political scientists—are coming to recognize the essential biological substrate of their own disciplines, and a growing number of biologists is actively concerned with the extension of biological principles and knowledge into the problems of small and large groups. There is a tremendous future for teaching biology in the social sciences and for effective team research in this area involving biologists; but the exploitation of these opportunities lags for a clear understanding of the relationships between the different areas.

Someone once defined ether as a noun invented from the verb "to undulate." In some respects, calling biology the science of

life is not too different. The concepts of life and organism and taxa will gain clarity, power, and dignity as we exhibit the essential entities or units with which we are concerned, their relations to one another at each level of interaction and the hierarchy of levels that they occupy, the mechanisms of interaction, and the like. It is important to recognize that biology tends to start at the organism (an individual, or a part, or even a unit event) and to move in one direction toward the subunits, where the physiological and analytic dynamic approach predominates (although it might be questioned whether the field is still not predominately descriptive) and, in the other direction, to the group or population or taxa, where the historical approach and the population concept with the associated notions of variance, is likely to dominate. Important errors have been made or deficiencies continued from the limitation of typological thinking to part of the biological spectrum and population thinking to the other, from the misapplication, either plus or minus, of statistical methods, from the neglect of boundary conditions, and the like. As I wrote in "The Organization of Science":

> There are striking uniformities in the trends of various segments of science; some having to do with emphasis on content deserve initial attention. Each discipline begins in a descriptive or classificatory manner. The subjects or entities of interest to it, the nouns of its language, are identified, described, tabulated, and ordered. This is the taxonomic stage, whether dealing with pure compounds in chemistry, minerals in geology, species in botany, structures in anatomy, or specific functions in physiology. Next is the static or structural stage, during which relations as quantitative as possible are established between these entities, and the verbs of state are introduced. Pythagoras' law, Ohm's law, Boyle's law, Bell's law, and Starling's law are fruits of such activity. Then come the dynamics stage and the verbs of change, when variations in time, space, and other conditions are introduced and the shift in relations examined. Integers preceded infinitesimals; electrostatics, electrodynamics; molecular structure, reaction rates; and the architecture of bodies, their action. Finally, with the holistic stage, the language is completed, the units in their variable relations are returned to the whole, the Gestalt is recognized, the planet or the organism returns to the center of focus.
>
> But this is too simple, for these stages apply not to each science but to each level within it. The natural entities examined by science are not ultimate indivisible units but are built of units which are themselves entities built of lesser units, often in a long regression. Man first grapples nature at a level dictated by the dimensions of his unaided senses, then pushes his way up or down through the layers—the astronomer to supergalaxies, the physicist to subnucleons—as instrumental aids enrich his senses and insight orders data. At the present rate of advance, at least in biology, scientists are moving a level per generation. Before this century, the organism had been dissected, structurally and functionally, to the organ level; during the first half of the 1900's the analysis swept on to the tissue, then to the cell; and during the second half, it is already pushing on to the organelle, particle, and molecule.
>
> Organ systems do not go out of bounds, however, when organelles come in. Each level advances through its own stages, development only being accelerated by the light reflected from other levels. Homeostasis is a holistic concept at the level of the organism. It gained in richness and precision from Claude Bernard to Cannon as the contributory organ systems were laid bare, as the actions of the liver and parathyroid and depressor nerve were recognized. It continues to gain as thiamine and cytochrome and cortisone enter the picture, and will so continue as ever more intimate mechanisms of membrane permeability, and enzyme action, and micellar architecture and gene reduplication are revealed. Similarly, behavior of the whole organism was studied for long at the descriptive level, while the nervous system was probed to nuclei and neurones and synapses, to action spikes and ion movements. Today, as these elements are recombined, the properties of neurone nets and potential fields and metabolic rhythms give more than promise of an understanding of behavior dynamics.

The new systematics, with its emphasis on population and on statistics and on directions in time, is essentially neglected in the above statement, but this is not really alternate to or irreconcilable with the other approach. In the field of populations and evolution, as of organisms and ontogeny, or of cells and protein synthesis, the identification of collective or singular units, the recognition of their variance, and the physico-chemical analysis of their mechanisms all play a role; and the steps of taxonomy, morphology, dynamics, and holism differ only in duration and intensity between them. The new systematics may well supply the kind of intellectual skeleton on which to order the previously over-

whelming descriptive facts of the old systematics, just as X-ray spectra breathed life and importance into the moribund accumulation of data and their classification in the subject of crystallography. It may even infuse new meaning, and therefore life and support, into the presently moribund museums—relics of the 19th century interest in collecting and classifying organisms.

This leads to a consideration of the organization of biology. This could be done in several ways. For example, the divisions could be in terms of objects of study, kind of interest in them, methods used, and objectives (such as application). In the more limited physiological area, the Survey of Physiological Science used a code for classifying research projects in which the main dimensions were: experimental variable, type of organism, organizational level, functional response, socioeconomic orientation. The Biology Council is organized primarily on the level scheme, with the headings: molecular and cellular, developmental, genetic and systemic, regulatory, and environmental and group. Some feel that this particular breakdown underplays the historical and population aspects of biology; and a primary dichotomy has been suggested, between physiological biology, with a biochemical, reductionist, and typological emphasis; and historical biology, with emphasis on higher integrated units, their interaction with other units and the environment, the population concept, statistical variation, and origin rather than mechanism. Under the first would be subsumed cell physiology, comparative and regulation biology, cell interrelations and interactions, growth, embryology, physiological genetics, perhaps caste determination. Under the second would come the new systematics, comparative human biology, physical anthropology, taxonomy, population genetics, ecology, evolutionary biology, paleontology, and animal behavior—presumably including the concepts of communication and information and signals, while leaving the power or energetic and material aspects in the other rubric. There is certainly room for argument as to the location of particular items; perhaps growth, development, and physiological genetics being most debatable. Other suggested ways of organizing biology are: genetics—sex, composition, metabolism, development, taxonomic relations, population dynamics, behavior; or organic evolution, reduplication of genes and chromosomes, sex—diploidy, stability—stable gene, homeostasis, ecologic equilibrium, the population concept—variance, means.

It is obviously impossible to organize biology according to a single scheme that would fully encompass various and sometimes alternative approaches. I have, therefore, attempted to incorporate in my own conceptual scheme as many as possible of the suggestions made by others. Actually, by the device of filling a table's cells, not only as a two-axis plot but as a multidimensional one, it has proved possible to incorporate a large fraction of all viewpoints. The remainder of this document is, accordingly, a split-up multidimensional table. The roman numerals constitute one axis which remains constant throughout. These represent the *levels of organization* and are as follows: I molecule, II organelle, III cell, IV organ, V individual, VI small group (band), VII species (potentially interbreeding population), VIII community, ecological area, IX total biota. The other axis, *A B C* etc., in the one case with subdivisions, represents the other category being related to the organic level. The categories so treated are *methods*, *biological disciplines*, and *concepts*. It was originally intended to make similar ones for the units, the relations, the problems, and the dichotomies of biology; but it seemed that when the analysis was carried through under methods, disciplines, and concepts the coverage was sufficiently complete to make the further steps unnecessary for this preliminary stage.

The headings under *methods* are: (*A*) identifying the elements, (*a*) tagging it; (*B*) modifying the element, (*b*) controlling other factors; (*C*) isolating the element; (*D*) measuring the element. There is really also needed an (*E*) referring to the making of a model and predicting and quantifying on the basis of it; but this does not lend itself to division along the level axis. For considering biological disciplines and biological concepts, three categories proved

satisfactory (rather than the four in my original table). (*A*) deals with structure or architecture, is concerned with spatial relations, with processes that have become fixed in material forms, and should ultimately cover structure as a type of information or negative entropy, and relate to the degree of determinism or stochastic freedom exhibited by the processes which produced it, and also the variance in its relationships. (*B*) is equilibrium, and is concerned with dynamic equilibrium or flux, and with organisms or orgs as dynamic entities. This also includes the notion of function, of interaction of entities and of responses to stimulation. The last rubric, (*C*) is concerned with history or origin, the problem of specific synthesis and of local entropy decrease.

It is also possible to group the six types of relationships between elements under these three rubrics, two in each. Under (*A*), structure, would fall spatial relations (including direction, distance, and containment) and hierarchical relations (containing, domination, preference, symmetry, and transitivity). Under (*B*), equilibrium, would come the operational relations (including attracts, loves, controls, signals, judges, studies, etc.) and the logical relations (including implies, negates, combines with, is alternate to). Finally (*C*), history, includes temporal relations (including sequence, movement, change, reversibility) and existential relations (including generates, destroys, imagines, spends, has common descent with). Other of the more general relations are placed, in each list, under IX—I fear without too much logic for this position in several cases.

METHODS

	A. Identify the element *a.* Tag it	*B. Modify the element* *b.* Control general variables	*C. Isolate*	*D. Measure*—ever finer units of mass, distance, time, and ever longer into past time. With precision use less material and disturb it less. Automatic recording (Brown), permanent record (photograph, kymograph), estimate of significance (statistics)
Molecule	I. Make tagged molecule Tracer-counter Immune test Spectroscope Spot and color tests Gasometric	I. Radiation—X-ray, light, proton, neutron,—rays Heat Sound, ultrasound Antibodies Enzymes (Breeding) Thermostat Barostat	I. Mass spectrograph Differential solubility (blood proteins) Differential distillation Chromatograph Breeding Centrifuge—macromolecules	I. *Mass* microbalance (quartz needle) Cartesian diver capillary respirometer counters *Time* oscilloscope thermocouple
Organelle	II. Breeding Staining Microscope: electron, UV, IF X-ray Microelectrode	II. (All above) Currents Substrates Drugs Salts Control: Temperature Medium Gases	II. Centrifuge Micromanipulation (pipette, needles) Sections (Linderstrom-Lang)	II.
Cell	III. Staining, histochemistry Microscope X-ray Microelectrode Functional test (tension) Permeability (tags for penetration)	III. (All above) Activity (fatigue) Hormones Stimulators Microsurgery Anisotonicity Time Chemostat Culture technique	III. As in II Culture Implant (cancer) X-ray, supersonic, etc., differential damage	III. (As above) *Space* histochemistry UV microscope microelectrode velvet plating culture *Number* RBC count bacterial count nuclear count

METHODS—*Continued*

Organ	IV. Dissection Functional test (liver, gut, circulation) EEG, ECG. etc. Manometers Strain gauges Tags for movement	IV. (Most of above) Surgery—remove, implant, section, reconnect, isolate Horsley-Clarke Vessel clamp Coleoptile and growth hormone Clinostat Perfusion Anesthesia BP holding device Heart rate control Deafferentation Decerebration	IV. Perfuse Surgical, isolation, nerve section, attachment (kidney) H. C. Lesions or stimulation	IV. Electric measures—oscilloscopes Chemical measures—tracers Mechanical measures—strain gauge Functional tests—secretion, response to hormone growth
Individual	V. Collecting Field observation—number, locus, etc. Taxonomic standards Surgery—Siamese twins Physical examination Personal history Ability measures Personality tests Free association Hypnosis Tagged animals	V. (Most of above) Diet Exercise Stress—emotional Conditioning, learning trials Infection Maturation—age Pure lines Constant age, sex, season	V. Siamese twins Tagging (banding, radioactive worm) Selection on some structure or performance Typology	V. *Dating* C_{14} tree rings, scale rings pollen counts fossils
Small group	VI. Field observation Interview Questionnaire Experimental groups Animal behavior	VI. Add or subtract member Alter communication net, means, speed, range, kind Enemies Symbionts Other groups Task Time Physical environment, habitat, light, temperature, water Chemicals—food, hormones	VI. Cultivation in fields, greenhouses, kennels	VI.
Species	VII. Ecology Biogeography Marine biology Statistics	VII.	VII. Latent structure analysis Factor analysis Panel interview	VII. As V
Community, Eco system	VIII. Sociometry Ecology Biogeography	VIII. Experimental evolution—introduce new species, chemicals, radiation	VIII.	VIII. As V
Total Biota	IX. General methods of science	IX.	IX.	IX.

DISCIPLINES

Molecule	*A. Structural* (architecture, spatial relations, negative entropy) I. Biochemistry—substances (nucleins, amino acids, chorophyll, etc.) Serology	*B. Equilibrium* (dynamics and adaptive responses) I. Biochemistry—enzymology, physical chemistry, reactions, metabolism Immunology	*C. History* (origin) I. Biochemistry—biosynthesis, template action, autocatalysis, adaptive enzymes Genetics—gene duplication, mutation

DISCIPLINES—*Continued*

Organelle	II. Histochemistry Cytology	II. Particulate physiology (micromanipulation)	II. Sex—diploidy, mitosis Virus development Microbiology—phage growth Somatic genes, plastids, microsomes
Cell	III. Cytology Histology Biophysics Protozoology Microbiology Immunity	III. Cellular physiology Biophysics Pharmacology Pathology	III. Embryology Growth Cytoculture Oncology
Organ	IV. Anatomy	IV. Organ physiology Biophysics Pharmacology Pathology Dentistry	IV. Embryology Comparative anatomy Comparative physiology
Individual	V. Anatomy, somatotypology, sex Physical anthropology Comparative human biology Factor psychology Personality psychology	V. Vertebrate physiology Plant physiology Human physiology Dynamic psychology Medicine Veterinary medicine	V. Comparative psychology Genetic psychology Embryology (Growth) (Reproduction) Gerontology Comparative anatomy Creativity—innovation, art (broadly)
Small group	VI. Cultural anthropology (e.g., kinships) Comparative human biology Sociology (roles, status) Networks, communication	VI. Group psychology Sociology (role playing) Cultural anthropology Animal behavior	VI. Experimental anthropology (Peru hacienda) History Behavior
Species	VII. (Comparative Anatomy) Taxonomy Population statistics (gene distribution) Immunity (blood groups)	VII. Cycles (population dynamics) Physical anthropology Functional sociology (economics, political science) Animal behavior Agriculture	VII. New systematics Population genetics Paleontology Anthropology (cultural change) Chronology (prehistory) History
Community, Eco system	VIII. Biogeography Ecology descriptive, quantitative Sociology—statics	VIII. Dynamic sociology Social psychology Ecology—food chains, climax, dynamics (forest, lake, etc.)	VIII. Ecology—successions Sociology (social evolution, new institutions) Anthropology (cultural evolution) Archeology
Total biota	IX. Resource problems—supply and demand, current and fossil supplies, and future expectations Area Energy Material Information (Zoology, Botany)	IX. Dynamic models	IX. Origin of life Cosmogeny Cassandraism

CONCEPTS

	A. Structure (spatial relations, fixed processes, negative entropy—determined or stochastic—to form it, variance)	*B. Equilibrium* (dynamic equilibrium, org, flux)	*C. History* (local entropy decrease, specific synthesis)
Molecule	I. Isomerism, stereoisomerism, strain theory, hydrogen bonding Atomic size, molecular size and shape Chemical specificity, enzymes, structure of protein	I. Mass action, flux, activation, energy, catalysis Reaction mechanisms (hydration, phosphorylation, peroxide, etc.) Contact regulation (solubility)	I. Template reproduction, autocatalysis, polymerization, branching chains, genes (mutation), adaptive enzymes

CONCEPTS—*Continued*

Organelle	II. Gene arrays, mitochondria—cyclophorase system Micelles, colloids (sol and gel), coaccervates, membranes	II. Permeability, ion transport Space effects of chromosomes, etc. Metabolism Contraction Excitability	II. Chromosomes Microsome, etc., reduplication Growth directing micelles, etc. Differentiation and reintegration (and below)
Cell	III. Cell Milieu interieur	III. Secretion Conduction, nerve impulse Hormone	III. Mitosis and meiosis—diploidy and sex Specification vs. totipotency Induction Retention Cell theory (all cells from cells)
Organ	IV. Tissue, organ, system	IV. Transmission Reflex Hormone Buffer mechanism Transport Field Drives Function	IV. "Entwicklungs mechanik," orthogeny Conditioning Imprinting Anabolism
Individual	V. Organism (org concept, individual) Environment Specificity The unconscious Consciousness, feeling Mind	V. Open system Homeostasis (physiologic constant) Stress (limit of homeostatic tolerance, factor of safety) Integration Gradient Adaptive behavior, sensation, gestalts The ego function Volition, freedom Wake—sleep Active—rest Well—sick	V. Biogenetic law Learning Remembering Creative behavior Ideation—new idea Goals Purpose
Small group	VI. Primary group Microclimate, climate (Coral reef, forest)	VI. The superego function Enculturation and acculturation Group dynamics Parasitism, symbiosis, host Infection	VI. Breeding selection Migration selection Gametes and sexual reproduction
Species	VII. Species, population concept Variance Ecologic niche Homology—analogy	VII. Predator—prey relation Population dynamics Symbiosis	VII. Natural selection Speciation mechanisms (isolation, etc.) Adaptation
Community, Eco system	VIII. Culture Language Phoneme Artifacts Norms Community Institution (several meanings)	VIII. Balance of power Supply and demand	VIII. Social and cultural evolution Goals
Total biota	IX. Web of life—in space Taxa (Hierarchy) Orgs—organization (Analytic vs statistical) Symmetry Element-set (class) Inside-outside, boundary "Organic" disease	IX. Web of life—in contemporary time (Mechanism—analysis) Attraction—repulsion Dynamic (vs static) Balance—golden mean Maintenance Function, activity Life—death (Quantitative) "Functional" disease	IX. Evolutionary tree Web of life—in past time (Teleology) Heredity—environment Holism and synthesis Development "Functional" to "organic" disease

CONCEPTS OF BIOLOGY

Condensed Transcript of the Conference

GERARD: You are all most welcome to this rather different gathering. Since it is sponsored by the Biology Council, I shall call on Paul Weiss, the Council chairman, to open the session.

WEISS: The Biology Council was set up several years ago—it went into operation about two years ago—as an agency of the Division of Biology and Agriculture of the National Research Council. Of those present, Drs. Gerard, Goddard, Mayr, Schmitt, and I are members of the Council. The group totals eighteen members, at full strength, covering, more or less, all areas of the biological sciences; each member is selected very carefully so as to represent multiple areas or disciplines. It is a deliberative group, with the task of exploring over the years just what has been going on in the biological sciences, irrespective of the various special interest groups that exist within those sciences, and to take stock of the whole perspective, not only in research but in its interrelations with education, publication problems, research financing, public relations, and so on. Pertinent issues spotted by the Council as needing closer attention are then formulated and delegated to appropriate task forces for further group study.

One of these task forces is the Committee on Educational Policies, which has done a superb job now for three years under Dean Phillips [Howard Phillips, Emory University]. This committee is studying the whole range of problems in the field of biological education, i.e., education for biology and biological education for the non-biologists, from high school through college and including adult education. The committee operates irrespective of any particular pet theories, employing the broadest possible perspective.

Now this group, the Education Committee, became disturbed, as many of us have, by the fact that biology is developing, more and more, in a strictly pragmatic direction and is losing, to a certain extent, its claim to becoming a full-fledged science in the true sense of the word, with a well-established theoretical basis and statements that can be universally communicated in interconvertible and mutually compatible terms. In other words, conceptual maturation has not kept pace with empirical development, and strains and stresses are becoming noticeable here and there due to this lag. The reason is partly to be found in the disproportionately low attention given to conceptual matters in education; hence the concern of the Education Committee, which is however shared by the Council as a whole. Characteristically, the Biology Council tackles such things by assembling a group of people to grapple with the problem, to discuss the situation before undertaking any serious investigations or making any formalized statement—in short, to talk things over.

So it was decided to have a conference of this kind, not on specific concepts in biology, of which there may be many valuable ones, but on the question whether biology indeed can and should stress concepts in the same sense as is done in other fields of science. If so, what can be done to make conceptual thinking respectable? Apparently we would have more thinking of this nature if it weren't just shrugged off, as it now is, in our empirically minded scientific activities. As we are getting further and further away from theory, this seems to be the time to see if anything should and could be done about this trend. These, then, are essentially the purposes of the Conference: first, to see whether these 18 Council members are correct in suspecting that there isn't enough preoccupation with basic concepts; and, second, if they are correct, what can be done about it. There is no urge to straighten out incompatibilities of thought in the biological

sciences or to coin concepts here as a group. After all, we represent but a small sample of biology, and the task is worldwide. The really basic problem is to see what can be done to make conceptual activity respectable, and perhaps to revitalize it. The Council would be very glad if practical suggestions were to come from this group, which we could then pass on down to the Committee on Educational Policies.

GERARD: Thank you. As Weiss was speaking, it occurred to me that one might think of the Council as Hercules, with his several labors. This is one of the labors and there is no doubt, whatever, as to which it is. It is cleaning the Augean stables. Just what we will accomplish or hope to accomplish, we can spend some time talking about ourselves. This is, from the start, an unusual kind of gathering. In every symposium that I can recall, the people were gathered to communicate to one another their specialized knowledges; we are not concerned with this, but with pooling our mature wisdoms. So there are no formal presentations. But I warn you now that, at the finish of these remarks, I shall start the proceedings by asking each of you to say a word about his general orientation and his views on the problem before us.

As to objectives, you can think of any number of reasons why we should try to clean out the Augean stables and introduce a little more formal intellectual order into biology. It would certainly improve teaching. It might make it a little easier to eliminate some of the dead stuff and introduce the new, and to point out gaps and areas of overlap in research. As Weiss has already said, it is very important, in the judgment of the Council, that biologists be encouraged to view broad thinking about their problems as highly desirable rather than as somewhat suspect or positively degrading.

In my judgment, one of the most important outcomes of such an examination of the conceptual structure of a larger discipline, such as biology, will be in institutionalizing it. As the numbers of scientists, institutions, journals, organs, and everything else continues to increase, there are only two alternatives—either let things happen as they will, or entertain some idea of the ways they perhaps ought to happen, and encourage them to happen in those ways. Should biophysics be a separate discipline? Should neurochemistry publish a new journal? What are the logical ways in which the great area of biology might be expected to divide itself in the future? Are the academic departments that have come down from the past now outdated, and should there be a new kind of organizational structure in universities? These are the sort of questions that will be answered by events, with or without thought.

Finally, I should also like to point out that, if we can see what are the interrelations between the parts of biology, we may be in a better position to expound to non-biologists what biology, as a whole, is. This would give a better, a more modern, and certainly a more realistic picture of what biology means. We might even get people to call themselves biologists a little more often, which would have great repercussions in bringing about public understanding and, through that, public support, and might steer competent students into the field. At present our selection of students is definitely an adverse one, in terms of academic standing, IQ, and the like; the average recruit to biology is of a rather disappointing level.

Now, the documents which you have are three: the initial working paper, with a somewhat formalized tabular organization at the end, which I prepared for the Biology Council; the rather expensive published article by Mainx,[1] in German-English; and the few pages from a career guidance booklet prepared by the Division, with its three-dimensional outline of subjects, methods, and levels (Appendix I). It is rather satisfying that, in all three of these, and quite independently, the same basic structure of the subject emerged. *If* we find that we can agree on a logical division fairly early, we will then go to a more detailed examination of some subheads or "boxes" of the table. I am not at all sure what will happen and, as indicated in my earlier letter, I have no great illusions of wrapping this all up in a

[1] See reference list, Appendix III.

nice package. Of course, if we do come out with a series of agreements, which can be put in the form of a formal statement, that would be fine; but we will, probably, do essentially one of two things—battle through the conceptual problems at a fairly general level, or, if and when this can be passed, start on some particular subhead for detailed examination.

This brings me to procedures. Logically, one should start with the great philosophical problems that underlie all thinking, including biology. I don't have to enumerate these, they include reductionism, operationalism, and so on. Then, at a somewhat more concrete level, we can get at the particular conceptual issues that are germane to the organization, content, and approaches of biology. At the third level, we can consider specific examples, factual arguments or interpretations, the meaning of words and terms. I suggest that we eschew rather vigorously the first of these. Once we get into these larger philosophical problems, we will get nowhere—everyone in the room has spent years thinking about them from time to time and has achieved a pretty clearly formulated Weltanschauung which is not going to change much. If we get into these arguments, we may never get out. Conversely, it will be excellent if we can get into the more detailed analysis, but we should not do so prematurely. That is, we shouldn't start with an example of something and then find, an hour later, that we've gone rushing down this bypath and spent much time analyzing *an* example which was cited initially only to illustrate some larger point. So if you are in agreement as to the general fairway down this course, I regard my job as Chairman is chiefly to keep the Conference steered in that zone.

As we go around the table, we should have some discussion of objectives of the meeting. This is the first topic on the circulated agenda. Then come three sessions on the role of conceptual analysis and the choice of coordinates. The coordinates are those which were implicit in all the documents, explicit in mine. One can think of *levels* of organization, from molecules to total biota, as one coordinate and of *properties* of the system as another. Are these the right ones? Is it useful to have a "level" coordinate and, if so, have we chosen the right units, the right levels, to talk about? Do "properties"—the architecture, the functioning, and the history of systems—form another good coordinate? A third coordinate is suggested in terms of methods, and another in terms of organization (disciplines) of biology. Will "levels" and "properties" of systems (or orgs) be the most useful ones for us?

Then should follow the analysis and subdivision of the coordinates agreed upon. Are the divisions in them, the units, the right ones? Is a division of biological properties into structural, functional and historical the best division? If not, what is better; if it is the best, then how far can we subdivide each? What does one mean precisely by the structural aspects, the functional aspects, and the historical aspects? Finally, having gone through a conceptual clarification, we arrive at the examination of particular boxes —of the squares or cells in the table. What do we want to say about them? Is it useful to go up and down the columns, or across the rows? What insights can result?

[The Chairman then repeated his request that each participant state his own views and orientation, pointing out that the group was selected to range from the physical and chemical to the psychological and sociological and, in effect, summoned the first witness.]

WEISS: Coming into biology from engineering, which may flavor some of what I am going to say, and on the basis of almost forty years' thinking about some of the principles of biology, I can best start off by declaring quite frankly that I feel there are a few basic things which could clear the air right from the start. That's why I cannot quite agree when you say we shouldn't go into philosophical questions, because I believe that a lot of the perverted concepts in biology result not from a fragmentation of technique but of viewpoint, that is, taking a *pars pro toto*, the different interest spheres determining the philosophy of the concepts. Just as they once did in physics. And I'd like to put this down as one of our major objec-

tives, to see if we can't clear the air along that line.

Let me illustrate what I mean. To a large degree, the physicist has ignored the individual phenomenon—the *item*—except as a tool for arriving at general formulae. The individual item was of no interest to him; it didn't have to be. Now some of the inorganic sciences, geology for instance, do have to be concerned with the individual item. The geologist is not only interested in the laws by which mountains are formed but he's also interested in the specific and unique structure of the Rocky Mountains or the Appalachians. To be sure, since this is a non-recurrent item, it becomes an object of natural history, rather than natural science. But some other branches of the physical sciences proper are also distinct from pure physics. For instance, the man who studies the structures of metals, or the meteorologist concerned with the morphology of a thunder storm, is getting much closer to biological objects than the basic physicist, because he is not only interested in the over-all laws within which nature repeats itself, but in the specific expressions and constellations of these laws in aspects of nature which are of sufficient interest, as a species, an individual, a cumulus cloud, etc., to merit a name; in other words—itemized phenomena which recur in essentially the same form.

The physicist, when he deals with snow or ice, is primarily interested in the transition from liquid to solid phase and with thermal and energy changes accompanying that transition; in this he deliberately disregards the shape of snowflakes. Recently I read a most exciting book about snow crystals, by a Japanese, which shows a remarkable beginning of interest in physical structure—in the many particular specific ways in which water can be transformed into ice. It presents the student of physical objects, perhaps for the first time, with problems similar to those we have in biology—the itemized pathway taken by the general dynamics underlying the phenomenon; in short, its mechanism. It is because we are interested in the *way* things happen, and not just in the general framework within which they must happen, that biology—I throw this out as a challenge—should not be compared with physics and chemistry, but with technology, with engineering, the application of physical and chemical principles to the determination of specific courses of events, where the nature of the course is the significant thing.

It is basically a question of focus of interest. You turn your interest in the direction of thermodynamics and say, simply, "This is the frame of reference within which things must happen but I don't know, or care specifically, how a particular thing happens." But the biologist has to be interested, in addition, in the specific way in which these things happen. In evolution it isn't a matter of saying only what *can* happen, but you are also interested in the very limited, finite choice of courses that evolution *has* taken and which are presented in actual species. In medicine, one is concerned with the particular type of illness, not just that 15% of afflicted individuals will die but why, in the individual case, the patient will 100% live or 100% die. I think this is where one of the basic difficulties in biological thinking has come in—this limitation of interest to either one aspect or the other. Biologists have often tried to extrapolate their philosophies from limited, fragmentary observation of one side of the biological world only. Finally, this is particularly true when they contrast the analytical approach, dealing with general dynamic principles, as in physiology, biophysics, and biochemistry, with such areas as, let us say, evolution or systematics or taxonomy, dealing with the items of nature, as if these were not merely two different aspects of the same continuum of phenomena. They then proceed further to confound the situation by failing to distinguish sharply, for instance, between such very different classes of "cause" as that a human embryo has gill slits "because" the fishlike ancestors supposedly had them as against the embryological statement that there are slits because some agent was there in the given embryo to spare out a hole and prevent a closure of the body wall.

It is thus that the two groups become confused and often bridge the gap between what basic physics, as they know it, can deal with on the one hand, and the structural aspect

on the other, by a purely verbal bridge, ascribing the difference to "organization," "complexity," or what not. These and similar terms are absolutely meaningless unless they are given conceptual solidity of some kind. And I'd like, later on, to choose a few of these, like the problem of organization, to see what can be done to get such terms out of the realm of vague literary meaning into the realm of scientific precision. Maybe they can't be satisfactorily defined, but people should at least struggle to give them objective connotations. This is the type of thing I had hoped would come from this conference.

[There ensued a preliminary flurry of discussion on "organization," which was deferred by the Chairman, who called on Dr. Wright.]

WRIGHT: I've been interested for a long while in this problem of relating the different fields. When I first went to Chicago about 30 years ago, there was a feeling there that my field, genetics, was distinctly an outlying one, important largely in animal and plant breeding. My problem there, as I saw it, was to try to show how genetics took one viewpoint of, and was really centrally related to, all the fields of biology. I made up a scheme of classification of the biological sciences [Table 1] which I used in my opening lecture in genetics for the whole period. It was a classification of biological sciences primarily at the level of organization, and, secondarily, by the similar problems that arise at each level of organization. It contrasted with the older primary classification by kind of organism as well as with primary classifications by utility or techniques, which I referred to as possible additional dimensions. Before it was finally published a few years ago as a presidential address for the Natural-

TABLE 1

A SCHEME OF CLASSIFICATION FOR THE BIOLOGICAL SCIENCES

Level of Organization		Description		Dynamics			Genetic aspect
Primary	Secondary	Climax phase	Secular change and reproduction	Persistence	Secular change	Reproduction	
Biota	World	Biogeography	Paleontology	Theory of biotic evolution			
	Local	Descriptive synecology		Dynamic synecology			
Society	*Traditional Society* Mankind Nation Special groups	Cultural anthropology	History	Sociology	Philosophy of history	Cultural cleavage	Genetics of behavior
	Instinctive society	Descriptive autecology		Dynamic autecology			
Interbreeding population	Species group Species Subspecies Deme	Taxonomy	Phylogeny	Theory of evolution			Population genetics
				Statics	Transformation	Speciation	
Individual	Colony Individual Organ	Anatomy	Descriptive embryology	Physiology of organs	Physiology of development	Physiology of reproduction	Developmental genetics
Cell	Tissue Cell Cell constit.	Histology	Differentiation	General Physiology	Physiology of differentiation	Physiology of cell division	Physiological genetics
		Cytology					
Gene	Linkage system Gene	Cytogenetics Genic chemistry	Nature of mutation	Physiology of persistence	Physiology of mutation	Physiology of duplication	Theory of the gene

From Sewall Wright. Gene and organism, *American Naturalist*, 1953, 87, 5–18.

ists, it took quite a variety of forms. It has seemed to me that perhaps in every course there should be at least a lecture showing the relations between that particular field and all the rest of biology and that these fields are not in reality separate pigeonholes. The student can easily forget, when he goes into the next classroom and deals with that pigeonhole and so on, that all are connected and that the major advances in science usually come at the boundaries.

MILLER: With Dr. Weiss, I believe it is important for us to outline our philosophic assumptions. I'm not sure that it's worth arguing about all of them. On issues like the different types of causality, operational definitions, the role of analogy in science, there may be a good deal of agreement. It might be helpful, early in the discussion, to find out whether we agree or disagree on a number of these questions, passing over those on which we agree.

It would also seem worthwhile to discuss method. In looking over the history of almost any science, one finds two popular positions. One is the generalist position; one is the particularist, or specialist. We tend to reward the second in science, certainly in America, more than the first.

In any field you can find representatives of both points of view. The specialist will be saying, "Here is a detailed description of a delimited phenomenon, or series of phenomena, which have these particular differences." The emphasis is on the difference, or disanalogy, from other phenomena. He may get his satisfaction by describing a new subspecies, or a specific tribe if he's an anthropologist. The generalist tries to find the analogies or the similarities among these different phenomena and will make generalizations based on analogy. A good example, from my own field of interest, is Freud's assumption that there are similarities between different types of behavior which he calls sexuality: physiological sex activity, psychological sex responses; mother love; aesthetic appreciation of music, perfume; and so on. Now, as soon as you make a generalization of this sort, the specialists come along and attack it, saying, "Well, obviously, mother love and sexual orgasm are not the same thing." Freud, of course, didn't mean that they were; he would have agreed immediately. On the other hand he would criticize the specialist who saw no generalities, just as general theorists in physics have criticized the specialists.

We will profit by thinking in both analogical and disanalogical terms at the same time. Otherwise, I feel that the general approach of the boxes as outlined in the agenda is promising. I hope we can get to them.

SPUHLER: My primary interests are anthropology and human biology. In both you have something of a state of disunity in that, by definition, anthropologists and human biologists are interested in what might be called the biology of man and, at the same time, his behavior. This latter has to do with the making and using of tools and symbols. I've been interested in the interconnections between biology and behavior, behavior and biology.

I have the impression that this conference is very timely in that the behavior side is much more receptive to biology now than even five or ten years ago. You hear very little now, outside of textbooks, about the heredity and environment controversy; the working people do not worry about this much more. I suppose the best way biology could be unified conceptually would be by cutting off the bottom level where the difficulty that was mentioned comes into a conceptual scheme that works fine in physics, and cutting off at least the top two or three levels on the upper end of your scale. We're getting difficulties of conceptual unity because we're trying to unify symbolic along with structural, functional things. I don't recommend this course, but I think we ought to consider whether biology is too broad as well as the problem of unifying it.

GERARD: Let me at once agree with that. The upper levels here would not be called biology by most people. It is my conviction that human behavior does stem from biology, and this is merely an attempt to indicate the spectrum in toto.

BRINK: The problem I have in mind mainly is how scientific principles and basic information can be so formulated that an individual can hope to comprehend at least the most significant features. Is specialization going to take us to the point where we

will have to be laymen in most areas of science and competent only in one limited area? Or is it possible so to formulate the basic ideas, so to condense the list of empirical statements, that one man with sufficient effort can comprehend their relationships and their significance and thus get a more unified view of the natural world?

Considering physics, chemistry, and biology, the question is: Can we only hope to be trained at the background, cultural level in all of these things or can we, if we are scientists, hope to have professional competence in these areas? Must we have limited competence here and there, let us say in applying physics and chemistry to biology, or is it really possible so to formulate the basic principles of these several sciences that we can hope to have professional competence all the way along.

You see, the question has a bearing on how you'd advise students (even how you might orient your own efforts, although it's rather late for that!) to spend their time, and especially whether you should encourage them to spread themselves or narrow themselves. And when, when do you advise them to narrow themselves? Nowadays, of course, we just say: "Let each man take his own course." If he does, he soon finds that biology is the stumbling block. It is not sufficiently condensed. It is still a long list of empirical statements, as mentioned in that pamphlet [Mainx].

The way out, it occurs to me, has already been mentioned. If one can find formalisms, such as physicists have used, for expressing the generalities and then handle the individual deviations by certain coefficients, this would be a highly desirable state of affairs. If we must deal with the individual case as an individual, historical occurrence, in order fully to understand it, then there is inevitably going to be such a list of empirical, well-established experimental statements that I don't believe it would be possible for a person to grasp all of them in detail and fully appreciate their total significance.

My general interest, then, is whether people who have the information feel that it is possible to utilize present formalisms or to develop new languages, new formalisms, that would be suitable for organizing large areas of biology, recognizing all the time, as physicists readily do, that you don't have just one formalism but you have as few as possible in order that one can grasp the significance of each one and its relationship to the experimental science involved.

WEISS: Could I make a factual correction? I think, Brink, you have misunderstood me, perhaps in too narrow a sense. I was thinking not only of such items as species, individuals, diseases, but also of such things as, let's say, a metabolic chain. We have to know the precise sequence of steps—the mechanism. That's what I mean by items.

BRINK: Then the question I was trying to get at is this: Is there a formalism in which the individuality of that particular chain of reactions would simply be an instance, a putting in of certain coefficients, if you will? Not necessarily an equation—maybe a model. I'd like to talk about this later when we get to particular cases. Perhaps in biology we need substitutes for mathematics where we talk more about relationships, but with less emphasis on the equational representation. For this purpose, electronic analog computer models may substitute for theory by representing multidimensional relationships in an effective way.

GERARD: You emphasized, in your statement, the problem of education for the professional student in biology and I know that Ernst Caspari has been interested in the problem of biology education for the general student.

CASPARI: I do not now expect to make any remarks about that; I would rather start out with quite a different question, the fragmentation of biology. It seems to me that here we run into a very difficult problem which is well known and which is quite different from what we have in physics and chemistry. And it is a fundamental difficulty, as evidenced by the fact that you can get your Ph.D. in zoology, botany, microbiology, and genetics, but you can't get one in biology. This is emphasized also by the fact that the biologists themselves, in their organizations and institutions, have a very large number of societies and feel themselves much more

closely identified with those societies than they feel themselves as biologists. And that is probably much more true for the biologist generally than for the people in the present group. In fact I think we really, psychologically, make up a rather untypical group of biologists. Most biologists feel themselves, first of all, as geneticists or as embryologists, and as Dr. Wright pointed out, an embryologist is often not very much interested in what is going on in the neighboring fields. As a result, in every one of the sub-branches of biology, sub-concepts have been formulated pretty much according to the need of the scientists at a particular time. Our difficulty is in putting these different textbook concepts, which have developed independently, together to form some kind of a whole.

There is another kind of difficulty. As Paul Weiss has indicated, when the physicist deals with snow and water, he takes that as a specific case of liquid-solid transformation and expects all other liquid-solid transformations to follow pretty much the same principle. Now, in biology we face the same situation but when we are doing any kind of work we have to do it with a specific organism and with a specific system. We are never really sure how far we can generalize about it.

It has happened over and over again that somebody starts generalizing on a case which later turns out to be a very abnormal one and which must then be discounted. What our present striving for unity in biology really amounts to is that people in very different approaches have reached similar conclusions. Thus we have seen that fundamental genetic mechanisms work in similar ways in different kinds of organisms. A generalization of this kind cannot be made by one man, but has to be made by several people working independently and then finding out that they have really been working on the same thing. This, however, increases by a certain amount the difficulty of bridging gaps between branches of the science, because the concepts with which the scientists are working are quite different, frequently at quite different levels of integration, and, therefore, the way they are related to each other is not always evident. The field with which I have been particularly concerned is physiological and developmental genetics, where some difficulty arises at the conceptual level from the fact that genetic concepts are formed at the intracellular level while at least in classical embryology the concepts were formed with reference to the developing embryo as a whole. Now it is apparently not impossible to bridge the gaps between the different levels of integration, but because of the fact the concepts have been formulated independently for the sub-branches of biology, it will probably require a remodeling of the concepts as they are used at the present time.

HOTCHKISS: I think I see a certain unity developing here already. In terms of the three-dimensional building [Appendix I], it seems that what we are trying to do is not so much to clean out a lot of empirical facts and rubbish as to clarify the various modes of entry. Various people have been using all sorts of different doors and each has in the main approached through one hallway with which he is especially familiar. We are really setting up a work detail to see how soon the partitions can be removed and what the whole building really does look like. Maybe it's an n-dimensional building, not a three-dimensional one. It's clear that we're not worried so much about the interrelations—everybody is agreed that the factual matter all fits together and is going to be harmonious enough. It's what happens to people confronted with the facts and the concepts that we're trying to solve. It's the disunity that comes over people when they try to talk to each other about it.

It may be as useful as anything to be a little autobiographical so that you can see the particular kind of stairways I've been using. Twenty years ago I got a degree in organic chemistry. What interested me at that time was organic reaction mechanism and I soon found out that biochemistry was full of this and eventually I began to deal with whole organisms. My interests have gone on to metabolism and trying to influence metabolism by genetic kinds of change.

I see myself as just one of those who has moved from the more exact kind of science

towards the more integrative kind. In my institute I long ago started thinking that the interesting people, to me, were of two types. Either they were chemists and physicists who wished they knew more about biology, or else they were medical people or biologists who wished they knew more about chemistry. Everybody who was facing up to the big issues was really trying to move along the dimension.

One often speaks of a simplified, one-dimensional scheme—mathematics, physics, physical chemistry, biochemistry, biology. Those who entered from a naturalistic point of view, if stimulated enough, tend to want to move toward the analytical sciences. And those from the physical sciences may have a sort of "social consciousness" which makes them say: "Well, the real problem is in this larger system, this higher level. I want to know about how sodium ions and chloride ions interact in dilute solution, but even more to know about them in concentrated solution, and then how this other ion will affect them—and so on." Similarly, a feeling of social consciousness moves biologists sometimes toward integrative aspects of civilization and culture as in anthropology. I am thinking of these two things as opposing trends that sometimes seem to set people against each other.

The only thing I want to say further is that there are forces, curiosity, and such things, that make a person explore border lines and bridges to other fields, and these, I should think, would be the kinds of forces needing more support. The opposed forces which tend to reinforce specialization and to narrow one are prevalent and powerful. When a person feels challenged or vulnerable he thinks: "If I make this statement, it will be scoffed at—I must be careful to keep my observations and remarks properly restricted." It's perhaps in these two things, which are probably in the realm of psychology, that one finds the interesting motivations that make it hard or easy for individuals to build bridges to one another, to be expansive and outward looking instead of inward looking. These are some of the issues that seem to me to be involved in the personal side equation. I think that what happens to a person confronted with biology is what we are really asking about; it looks as though everybody wants to be concerned with this.

GERARD: There was a vigorous nodding of heads as you brought that in.

GODDARD: I think I have been motivated, first, by my great difficulty in learning languages and by the early realization, when I had to teach freshman biology, that the students were required to learn more new words in a year of biology than in a year of French and German! In my own education, of course, I had a lot of separate compartments thrown at me but in commencing freshman teaching I rather revolted against this. I am looking at conceptual thinking now primarily from the point of view of teaching, even to the freshman student.

Let me give you an illustration. The invertebrate zoologist works out life cycles for different organisms and gives special names. Now I've found great difficulty in learning enough about the life cycle of malaria to be able to teach it in a class; it's just difficult for me to remember the words! You'll find that the people who have worked out the life cycles of the algae have coined a whole set, the mycologist has worked out a different set, and neither set jibes with that of any particular group of invertebrates. I tried, in teaching the freshman course, to present the basis of sexuality in a simplified scheme, whether uniparental or biparental, and got rid of most of the terminology. I even got this into the laboratory manual, and about that time left the campus for a year's sabbatical. During my absence, two very competent biologists reintroduced every single term which I had taken out and destroyed the whole general approach. They are both good biologists, but they were completely concerned with the particular and with verbalisms.

Now, if I were to work this thing out myself, I would be concerned first with the conceptual framework of biology in terms of the freshman course, so that the student has a chance to see that there really is some structure to biology. I would like to give you another example, which is dangerous in the presence of two geneticists, to say nothing

of Dr. Simpson. For a long time I found, in general biology, that genetics was tacked on at the end of the course; in fact, for a long time I wasn't allowed to teach genetics in biology because that was done by a geneticist. When I got a course that I completely controlled, I decided that I ought to give the genetics, too. And the thing that interested me was that if you took genetics away from the end of the course, if you brought in the concept of heredity the very first moment the students learn that organisms have parents, and if, the first time they hear about a chromosome, they learn that chromosomes have something to do with heredity, you find out that genetics runs all through the course.

So the first thing I want is to try to separate out, for the sake of teaching and the next generation, the real generalizations in biology, state them in simple terms, and then recognize that they are applied to specific cases, of which the student is asked to learn only a few examples. He'll find in the literature all the special examples he needs if for any particular reason he should be taken to it—and in most cases he won't.

Another part of the problem I have been struggling with for the last two and a half years as editor of the *Journal of Plant Physiology*. Here I find it almost impossible to get an author ever to state a generalization. It is not a matter of having generalizations turned down by the editors; the background, modes, and fashions in the field are such that authors are afraid of them. Our articles are extraordinarily specific and the relation of the particular problem to the general area of science almost never shows up in any of the articles that come to my desk. The journal I edit is the oldest, richest journal in plant physiology in the country, and has the largest circulation. I think it's one of the best journals of plant physiology in the world. In spite of that, from a conceptual sense, it's very mediocre and, I think, fairly represents thinking in this field.

So my own approach to conceptual thinking in biology is, quite frankly, at a very primitive level. I am thinking of generalizations, and the simplification in terminology that can result, from the point of view of teaching. And I have a feeling that if we could ever get for our students a group of textbooks that were written this way, and courses that were taught in this way, biology wouldn't be nearly as difficult for the person who comes in from chemistry and physics as it is at the present time.

SIMPSON: I was terribly confused, I must confess, even with the preparation sent to us to do, as to just what was expected to come out of this. And I'm not sure but what I'm equally confused now whether we're thinking in terms of what the biology curriculum should be; whether we should have professors of psychology or physiology or genetics; of precisely what *the* principles in biology are, or what *a* principle of biology is; or just what. Perhaps the impression from what's been said before is that we're thinking in every conceivable sort of terms. If so, I have a feeling that we're not going to get very far. I trust that I am wrong.

Certainly unless we do have something concrete in mind I'm not at all sure we can select. Because you have different kinds of concepts in science, depending upon what your fundamental purpose is, not just one system of them, not even a system of coordinates that can be used to overlap. For instance, this tripartite division into structure, function, and history. In orderly thinking that is certainly a useful one—I haven't the slightest quarrel with it. On my own side I am primarily interested in history, and would tend to stress the historical side of those three things.

But then, stop to think. What does it mean—interest in history? You have to be interested in the history of something. You have to describe, or specify, it somehow. In other words, go into its structure, whether it's literally the morphology of organism or the structure of population. And then if you start thinking about the structure, this is perfectly meaningless, especially dealing with living things, unless you think about the function. So that rather than being an actual scale which you can use to set up one part of your set of boxes here, this structure and function and history is something that would apply to every single compartment in the boxes—at least it should. Nobody

studies history without studying structure and function, because he can't, but you can study structure in a small way without going into the other things.

Perhaps what I've been leading up to here is sort of plea for anarchy—I hope I'm not just being subversive here, but a certain amount of anarchy in biological concepts or classifications seems to be a very good thing. I'm not at all sure whether trying to conceptualize along a definite number of dimensions, even *n*-dimensions, is fundamentally going to get us any place.

One or two persons have been a little autobiographical. I'm one of those who came into biology from a quite different science, from geology. I still consider myself a geologist, at least as well as a biologist. The work that I've done seems to me to be a rather unified body of work around a sort of central core, but there isn't any name for that core, there isn't any category, there isn't any box. I don't feel I'm moving from one room to another and one box to another, but I can't imagine exactly what set of coordinates to apply to my own work. Well, for me this is a good thing—I suspect it might be a good thing for biology in general if there were a lot of anarchists like me.

GERARD: Your main point, indicating the interrelation of these coordinates, structure, function, history, and so on, is obviously valid and this is the kind of discussion we will have to get into, as we go along, if we decide we want to do any kind of ordering of terms.

WEISS: Could I say a word on principle here? It would be a misunderstanding of the aims of the Biology Council in arranging this conference if we were to aim at any one particular and purportedly superior type of concept, work it out *in camera*, and then spread it as gospel or royal formula of concepts in biology. The thing we want to do, if I may point it out again, is merely ground work, simply exploring whether something could be done to make preoccupation with concepts respectable again for students, faculties, and authors; as Goddard points out, to grapple with these problems; to encourage investigators to concern themselves with the inner relations between the tremendous number of items and the confusing diversity of the biological sciences in such a way that they will have at least some fixed points of reference. For that reason I don't think it is up to us here to come out, or aim at coming out, with any set of boxes or any single set of coordinates at all.

[There ensued a brief exchange concerning the extent of agreement in the statements by Weiss, Simpson, and Hotchkiss.]

SCHMITT: I feel like a Texan about this—I don't want to be boxed in. I have personally very little interest in systematics as such; they are to me usually rather boring. One can convey very concrete ideas if one says: "Weigh this thing," or "I weighed it and it weighed 10.72 kilograms." On the other hand, if one says, "This is an acrid smoke," or "This thing is beautiful," it stimulates concepts in our minds which depend upon our past history and our physiological state at the moment. My purpose in coming here was not so much to get ideas of systematics, logic, or philosophy as just to be associated with what I consider to be a very interesting group—and to discuss concepts. I think one of the important things that will emerge from this group is the opportunity to conceptualize along lines which perhaps normally we wouldn't be stimulated to do.

At M.I.T. we have been concerned about the flocking of students to certain disciplines like electrical engineering and physics, whereas certain other disciplines, such as biology, are chosen by relatively few students. Further, when one looks at the statistics he sees that, over the last ten years, the number of students majoring in biology at college has been reduced some 50% during the last five years, whereas the total number of Ph.D's in biology has increased. This decline is a serious matter, especially in view of the fact that within the next ten years the total number of students applying to institutions of higher learning will double, requiring a correspondingly larger number of well-trained teachers. This is symptomatic of something. I think that one of the practical things that comes out is just what has been said around the table already, that biology as a discipline

has been so fragmented that prospective students no longer have a clear concept of its content. When you say "biology" it doesn't mean any special thing to a student except perhaps cutting up frogs, classifying butterflies, birds, etc.

I think that biology is in a period somewhat like that of classical physics before the era of quantum theory and knowledge about radioactivity and atomic structure. We may expect to see a great increase in our understanding of how energy is coupled in the macromolecular machinery of tissues and cells. I think we are entering a period in which not only the practical applications in medicine but also theoretical, conceptual advances will be very great. I suspect that one of the important advances will come in the consideration of the relationships of highly organized systems to each other in a manner now undreamed of. I think that system theories and a study of interaction of the components of systems upon each other —theories about the communication of information between macromolecules and parts of macromolecules—will have an important impact on the thinking of the next generation or two.

REYNOLDS: If I have a specialty, it's in the study of means to express biology to non-biologists. In a way the Office of Naval Research is a laboratory for this kind of activity and my particular job lies in trying to see that money goes to the support of biological research, often along a path that consists of crossed Nicol prisms. I have to defend individual research proposals, first before a group of scientists in other fields, predominantly physics, and then before a group of naval officers who will have had, at best, some engineering training. I think it is interesting that our office has been as successful as it has in supporting fundamental research in biology. In the course of the last ten years we have explored a great many different ways of getting this kind of thing across. Most of it turns into conceptual appreciation of the thing in the first place.

I'd like to go back now to two or three things that others have mentioned. Goddard pointed out that papers coming in to scientific editors seldom have any statement of relationship to the scientific environment. This is the problem that we almost always face, in receipt of proposals from scientists. While this is not too bad when presented to an advisory group of scientists in closely related fields, to manufacture a story which will carry conviction to a non-biologist presents a certain number of problems. If the individual making the proposal does make any statement about a relationship of his study to the rest of the world, it is likely to be an absurdity such as: "This is central to the problem of developing a cure for cancer." This has, in fact, been such a problem that I have asked people, in submitting proposals to us, to refrain from saying anything about the relevance of the study, so frequently are they more or less *non sequitur* statements of utility.

Our big problem is that this kind of expression of biology to non-biologists can't be educational in the usual sense that you expect it to stick, particularly if you haven't got a long time to apply it. Very frequently you must, in a statement so short that people will listen to it, give a reason for thinking it is interesting. I want to give one example of this kind. We have a Program Council, so-called, in which I have to defend each study that my Division is planning to support. Sitting around the table are four or five physicists, two naval officers, a mathematician, a psychologist, and a chemist. What I have to do with these studies is to show that what is being proposed is really interesting in a general scientific sense. The application, the utility, is usually of secondary importance. This is a very difficult job. What I try to do is to tell them an interesting story about a related field of biology, enough to get their appetites whetted, and then say: "Well, this, of course, bears on that." This usually gets it through, but it isn't completely satisfactory. In other words, we've got somehow, in the individual instance, to show the Navy that a study in Drosophila genetics is important to them, and frequently it has to be done in the individual case rather than in terms of broad fields.

MAYR: My interest in this conference arises primarily from two difficulties with which I have been confronted in my scien-

tific career. The first arises from my contacts with other biologists, such as geneticists, embryologists, ecologists, and so on. We all belong jointly to biological societies, but I have wondered sometimes just what it is we have in common, what makes us biologists. And I have felt sometimes that if you would take a group of diverse biologists, for instance the people sitting around this table, and say: "Will you, each one of you, put down the five most important generalizations that you derive from your work, or state what you consider the five most important concepts that have arisen out of biology," it would be a fairly safe guess that there would be comparatively little overlap. Each would have one or two that would overlap with some of the others, but there might be quite a few statements that are unique, that only one of the group considers the most important. So the first interest that I have here is that, somehow or other, I haven't quite reached the point where I see clearly what the particular features of biology are that are the more important generalizations. It is for our own understanding, for our own particular work in specialized fields, that we would like to see where we personally stand in this more general framework.

The second difficulty—I spoke of two of them—becomes apparent in our relations to non-biologists when we discuss with them what is biology and when we try to find out where does biology as a whole stand in relation to neighboring fields. I would like to cite just two experiences that I've had. One, I read not long ago a book by a well-known chemist in which he makes the statement—I hope I don't misquote him—"Biology will become science as soon as it becomes biochemistry." Now that statement shows that here is a man, an intelligent man, a well-trained man, one who has had many opportunities to discuss things with biologists, who obviously doesn't understand biology at all.

I don't want to single out the chemists, so I'll say something about physicists. I have had a number of conferences within the last two years with physicists who have become intensely interested in the subject of evolution. Niels Bohr, I think, brought up some years ago the question whether a phenomenon such as organic evolution could be expressed in terms of chemistry and physics, whether it obeyed entirely the laws of physics or whether there is something else involved that lies outside the known realm of physics. Well, in these discussions it has become very clear to me that the thinking of physicists is, on the whole, exceedingly different from that of biologists who work in the evolutionary field. The physicist finds it difficult to comprehend the uniqueness of so much that happens in biology, whether it is individuals, species, or certain biological events in animal behavior, ecology, and so forth. The physicist normally does not think in terms of unique events—he thinks in terms of his physical laws, he thinks typologically. Now, in addition to the technological or engineering bridge between physics and biology, which Weiss has pointed out, I think there is a second one which comes from the study of physical laws as statistical laws. Such things, for instance, as Heisenberg's uncertainty principle and similar developments in modern physics establish a very definite bridge to biology because here also individual events may very often become quite "biological." I remember a discussion I had with the physicist W. Pauli in which, somehow or other, he just couldn't get a particular point when I spoke of a gas as having such-and-such properties, until he finally said: "Oh, you're talking about *individual* gases!" As soon as he got that, he got my reasoning.

I think that for all these reasons it is a very good thing for some of us to sit together and just think a little about the generalizations and concepts that come out of biology in order, perhaps, to permit us to talk a little more intelligently than we have in the past. As far as procedure is concerned, I'm a little worried, as I notice several others are, about being fenced in by a scheme, by a conceptual structure which is, already, a rather advanced synthesis. A deductive procedure like that seems to me a less appropriate basis for our discussions than a more inductive way, studying the importance of individual concepts in various fields and then trying to see

how they fit together eventually, whether they make a particular structure. I don't say that this is necessarily the best approach, all I say is that I would, personally, like it best.

MILLER: There have been two or three references to being boxed in. I think that mavericks in general get a lot of fun, instead of grazing in fine green grass, from jumping over the fence and going into another field, but I'm surprised the notion of boxes has been devoted mainly to the restrictive or restraining aspects—the outside of the box rather than its contents. I suspect that most productive individuals have, throughout their scientific lives, hopped in and out of various boxes, and take great and proper pride in the fact that they see connections between one box and another. But indicating coordinates, so that you know where you are in latitude and longitude, is not necessarily restrictive of motion. Like the rest of those opposed to boxes, I am hostile to any concept that might "box me in," but I am not opposed to latitude and longitude, as such.

WRIGHT: My own approach is precisely that students often are boxed in. They study embryology and go on to genetics and forget all about embryology. The whole purpose was to lay down a system of coordinates so that one could draw arrows and show the connections at boundaries. Because you can't get half a dozen dimensions on the blackboard all at once, you have to make several schemes and bring out the interrelations. I was especially concerned with showing the relationships of genetics to other fields—that was the whole object of the scheme.

WEISS: I think, like in the play by Pirandello, "Six Characters in Search of an Author," to a certain extent we should let the thing just shape itself from our discussion here. On the other hand, referring back to one of the things Goddard said, our terminology is terribly overloaded, and the problem we might have to grapple with is that we are using in biology so many terms that stand for nothing; they are just empty words. And this is, I think, the most confusing thing at present, our greatest deficiency, conceptually, that we tolerate and use terms irrespective of whether they stand for something or are just a surrogate to further analysis.

MAYR: We have also a terrific number of facts that stand for nothing conceptual. My daughter, as a sophomore in college, complained that in comparative anatomy she had to learn the names of eighty muscles of the mud puppy (Necturus). She didn't, and flunked her lab test, which delighted me no end.

Some years ago when I was in charge of one of the exhibition halls at the American Museum I introduced the rule that not a single exhibit should be placed in the hall that did not illustrate some biological principle or generalization. This was one of the hardest things to put over on some of my fact-happy colleagues. They said, "Well, this fact is in itself very interesting." I said, "Now, what does it really teach you?" At present we have too many concepts, too many terms, that do not have a factual content, and we have too many facts that do not have a conceptual content. We should try, particularly in teaching, to do what Goddard says and go to things that have both.

HOTCHKISS: It seems to me that these anxieties about being boxed in come from the function of language. A symbol, a term, when applied to something may have either of two distinctive purposes or meanings. One is to say, "This is like these," and the other is to say, "This is not like those." And we're hampered by this bivalence; the specialist wishes to keep alive the differences (even non-existent ones) and isn't quite satisfied to use the term from another field. When you want to deal with the harmonies and community of thought between one area and another you like to reduce the terms and use those with a general basic meaning. I'm not sure, though, that this kind of assembling under broad terms, indefinitely continued, doesn't tend to result in one kind of words that Weiss feels are meaningless.

CASPARI: I'd like to point out that quite a number of meaningless terms have their origin simply in the history of biology. The larval stages [of trematodes] which Goddard

spoke of, all of which have names of their own, were originally described as distinct organisms and only later was it established that they are really larval forms. The story with muscles is quite similar. To name these muscles did have a scientific validity around the 1850's. The difficulty is that once we've named it, it becomes established and names are transmitted from generation to generation. There is certainly something to be said for thinking over these words and seeing which ones can be relegated to a dead language.

GODDARD: As an illustration, the word "sperm" has a perfectly definite meaning for all zoologists and botanists, yet you will find in different groups of organisms probably twenty different terms for it. My point is that where there is a general term with content and precision of meaning it is far more useful for teaching purposes than is the specific term. If the specialist, working with red algae or rusts, insists that the sperm must be called a spermatium, it doesn't concern me. It does concern me when this becomes an essential part of the general course. I agree that there are terms in biology, as Weiss has referred to, that are meaningless because they don't refer to either a structure or an operation. These we must get rid of. And, as Caspari points out, we don't slough off the things which were historically valuable but are no longer of value in the teaching of science. We are carrying a total, instead of a selective, inheritance, which becomes impossible from the student's point of view.

Finally, I would say that mere quantity—making a student learn eighty muscles, for example, in comparative anatomy—probably obscures the real generalizations and the real conceptual framework which could have been taught. Proper teaching could have done something else but instead all the time was spent on language drill. You see, I'm coming back to the statement that the teaching of certain kinds of biology has become the teaching of a language. I feel that most biologists have almost no understanding any more of what a good taxonomist is really doing. They think that the taxonomist is concerned only with names, for taxonomists have been very unsuccessful in communicating the systems which they have erected to the rest of biology. They communicate with each other very well and with non-taxonomists extraordinarily badly. This is actually one of the great difficulties in finding financial support for taxonomy. Part of our job, I think, is to induce cross-communication and I think we can do it if we cut down our terminology and can realize that it is not a goal in itself.

WEISS: I want to add, just for the record, a third type of term with which I think we will later have to deal. These are not the terms that are either superfluous or vague, but the ambivalent, particularly ambiguous, ones; that is, identical designations with different connotations. For instance, "growth" in botany, includes water uptake and mere lengthening; in zoology it means protoplasmic reproduction. If you know the content of the terms "growth A" and "growth B," that's fine, but as soon as you begin to operate across the board as most biochemists and biophysicists do nowadays, and equate the two just because they carry the same verbal label, then you get into trouble. A lot of the waste effort in biology, of research funds and otherwise, is traceable to this type of thing.

There are any number of examples. "Differentiation," between pathologists' embryologists, and physiologists. "Control?" What does the word "control" mean? "Hormone"—what does the term "hormone" connote? Is carbon dioxide a "hormone?" What's the sense of such questions as, "Is a virus a gene? Is a gene a virus?" There are serious scientists who waste time on this, and editors sometimes print it! Don't all of these terms have meaning only within a defined frame of reference?

I don't think you settle it by setting up a word as *the* characteristic one, you settle it by making biologists in educational systems conscious of what the term stands for in a particular set of phenomena and create this habit of considering terms in the proper perspective.

MILLER: Two further brief comments about procedure.

In many ways the goal of this group is

taxonomic, not of species but of ideas. For example, there are dictionaries of biological terms and synonyms which could be viewed as an effort to find the terms which are redundant, unnecessary. Donald Campbell, at Northwestern, found over two hundred terms which dealt with the notion of storage of information over time, many of which, used in different fields, obviously meant almost the same thing and some of which meant very different things. One question of procedure is whether we might not consider the value of efforts to locate species or categories of terminologies which are essentially similar and then indicate to what extent they are different, and whether or not these differences are important.

Secondly, I would agree with Dr. Mayr that the most productive thing for this particular group might be an inductive effort, perhaps by having each of us list five generalizations and then talking about them. The particularly fascinating thing about this group is that each member has a quite different background of experience. To go through the processes of arriving at generalizations might give us a sense of either the futility or the importance of this activity, rather than accepting, a priori, any group of coordinates. But I would hope that ultimately we might try to find good coordinates, subject to complete review at 9:00 A.M. tomorrow and every morning thereafter.

MAYR: You might not realize it, but the first and second steps of your procedure are the same as those used by the taxonomist because he recognizes a series of steps, sometimes referred to as alpha, beta, and gamma taxonomy. Alpha taxonomy is the definition of units, while beta taxonomy is the combining of these units into a system of classification. (Gamma taxonomy, the study of the biological meaning of all these units, wouldn't have as much application here.) So, if you say we should follow the inductive approach first and get the concepts and then see if they fit into a scheme, that is precisely the taxonomic procedure.

[The chairman at this point summarized his impressions of the morning's discussion and set the stage for the afternoon in the following terms.]

GERARD: As I listened to the discussion it seemed to me that there are some five or six things that form a fairly logical series for discussion: (1) the issue of anarchy and direction; (2) the objectives; (3) the relation of the model to the instance, of the type to the individual, of the generalization to the particular; (4) the actual procedure of the group in the conference; and (5)—I will state it most generally—what kind of order do we expect to attain in our efforts; what dimensions, if any, are likely to prove of use?

I shall add a word myself on the first. For several years I was chairman of the Evaluation Committee for the College at the University of Chicago. I recall we spent one whole afternoon on the question: Do we or do we not wish to indoctrinate the students? This is a nasty word and we all started off by saying, "Of course we don't want to indoctrinate anybody with anything!" but we ended up by agreeing that if we didn't do a good deal of indoctrination we wouldn't be doing anything.

This business of anarchy versus direction is itself an example of how easy it is to set up antonymic positions and to make an either-or issue. We obviously cannot tolerate complete anarchy and we equally cannot tolerate complete regimentation. The question is, to what degree is it useful to get things formalized for better communication rather than to start with chaos. There are two issues here: first, whether individuals or groups should attempt to exercise higher authority over their fellows—and we all agree that this must be eschewed at all levels; and, second, should individuals or groups attempt to exercise leadership in helping their fellows do something they want to do and I think we would be throwing out most of what we wish to achieve if we eschewed that also.

I give you an example. We are in general agreement that much terminological detail that, like barnacles, impedes the movement of biology with its history, should be cleared away in favor of a richer and more exemplified conceptual framework. I think we would be remiss in our duty if we did not come out and say that we believe this. This compels no one to teach differently but it does give

guidance to those who want it. We have not merely the right but the obligation to give guidance.

Exactly the same kind of problem comes up in the conduct of a meeting of a small group like this. What degree of structuring is optimal? I shall not make this decision for the group, but I suggest that, if we start completely unstructured we will end up after three days of fairly exciting arguments with the uncomfortable recognition of having said all these things before; indeed, this is likely to happen no matter what we do. We will get further if we start with *some* kind of organized procedure, whether the chosen structuring is the best, truest, and most useful or not.

When I went from the relatively simple statements, offered by colleagues of the Biology Council, to the actual job of erecting a conceptual structure and of allocating specific contents, notions, ideas, methods, and categories, I found myself involved in much hard work. No doubt this job done by each one here would have come out rather differently; but I have equally no doubt that basically they would all be pretty similar. Now, if that second assumption is wrong, we want to find it out quickly; and if it is right we want to find that out also, and get on with our job.

The first item for the afternoon will be some discussion of procedure. I trust we can agree that we are not trying to ram anything down anybody's throat, individually or collectively, and that we need not argue the morals of anarchy. We should then agree on objectives, although not in great detail, and then get on to some particular conceptual problems, which are very much our business. This issue of the general and the particular model can be exemplified in terms of words and terminology or in terms of lists of concepts; both might offer useful ways of proceeding.

[The group reconvened after lunch, and entered upon a consideration of objectives.]

GERARD: How many think that we will be content if, at the end of three days, we go our ways with this experience behind us? How many would hope, beyond that, for some kind of distilled wisdom to which we might subscribe and which conceivably would find its way into a report? If we strive toward that more tangible end, what would be the objective of such a report?

WEISS: May I say that, even without a formal report, the five of us members of the Council are going to listen very attentively to the general climate of opinion and draw conclusions to be evaluated by the larger group. So even those who go home and feel they've only had a good time should not feel that it's been entirely wasted. I'm certainly trying to get the general temper, because if there is no feeling here that something along these lines is necessary, we'll quit right there. This is essentially a hearing for the guidance of the Council.

GODDARD: I would say, first, that though it may be an extravagant use of money I can't help but feel that my own education is being much furthered by this conference. I hope it doesn't stop there, that we are willing to go on record. If we can write any report at all it should be published and not just filed with the Council. Whether this gets published after it has been through the mechanism of the Biology Council as a whole, and so on, I'm not concerned. As to the objectives, my own feeling is that you rarely reform the existing generation. The first objective as I see it is an attempt to get at the people who teach biology and who write textbooks of biology. If we have the kind of biological teaching that we want, you don't really need to worry about the next generation.

What worries me is the propagation of the biology of about 1850–75 through the generation that's going to school in the 1950's. The correlation between what is happening in the best biological research and in undergraduate teaching is very bad, indeed. If you walk into a class of comparative anatomy or plant morphology or many others one could pick—I think it is less true

of a good course in genetics—you might almost think that nothing has been done in research for the last forty years and that nobody was working! As far as I am concerned, my primary concern is how it [this conference] can help me in working with students, whether doctorate or freshmen. And I hope we don't stop with just having a good time together.

I don't think we need to discriminate between levels at all. I think, however, that we ought to try to phrase the report in simple enough language that people who are not, let's say, versed in philosophy or modern communications theory will be able to get something from the report.

I think Mayr made a very interesting suggestion this morning that each of us try to formulate very general concepts pervading a lot of biological knowledge, bring them here, and look at them. Because I think that there is more of real theoretical basis in biology than we normally recognize. The advantage of such a program is that we thus make the special case under the general rather than giving all the attention to the special. Nobody is foolish enough to think that these general things will themselves spell out all the details of the life cycles, structures, and behavior of each organism. But if each organism becomes illustrative rather than an end entirely in itself I think you've simplified the educational process very greatly.

GERARD: Are you suggesting, then, that this document might come out with a statement of certain important general principles of biology that deserve emphasis in teaching biology?

GODDARD: Yes. I'm not even sure you would have to mention the word teaching. I think if most teachers knew what these were, it would get across.

Just to take one case in point, a very good statement of the matter of biparental inheritance and sexuality, and the fact that there may be other methods of transmission of hereditary information besides the gametes. I am thinking of the transforming principle—I don't know whether that and transduction are the same thing or not. Then one can go on and give examples of this sort of thing. For instance, botanists quarrel over whether asexual reproduction and vegetative reproduction are different things. They may or may not be different structures; the interesting point is that you are dealing with uniparental inheritance and not biparental inheritance, and that there are all kinds of mechanisms of uniparental inheritance. If you understand what you are talking about you then pick out whichever examples happen to be current and available and which are a part of the student's common experience. But it is a disgrace to make students learn about all the different kinds of spores and gametes as if each were a distinct phenomenon. I have a feeling that if we really formulated the ideas ourselves in clear enough form so that they could pervade undergraduate teaching, the same ideas would be effective for people at the very mature research level, outside their own areas.

WEISS: I'd like to say, though, that we are several steps removed from the level of application. I deliberately pointed to the Committee on Educational Policies, because they are looking to us for guidance along that line. They are, right now, engaged in what promises to become a very effective attack in the general direction of improving curricula and teaching and I think they should have a body to look to, not to prescribe concepts as such, but to make a case for greater attention to the conceptual formulations of biology. I think a statement along those lines to the Committee would certainly be very useful. They are looking to us for this.

PAULSON (Exec. Sec., CEP): May I say one word? Not only the Committee on Educational Policies but also the teachers from the secondary school level right on up are looking for exactly this kind of help and assistance. I've sat in on a number of conferences involving educators at various levels and various disciplines in the past few months and there is a feeling, partly stemming from this antagonism between academicians and educationists in the narrow sense, that the academic people are so involved in their special interests it remains for the educator who is interested in

the bringing up of young people to formulate these general concepts. They don't know where to turn and would welcome, with open arms, guidance, from those who know best what they are talking about, just what sort of things ought to go into courses, what approaches and attitudes to build, what biology teaching really ought to accomplish. This would, perhaps, go over even more readily at the secondary school level and might eventually permeate some of the college teaching which, as Goddard has pointed out, is fairly far behind the times.

WEISS: I'd like to say, again, that we are several steps removed from the level of application. We cannot formulate here, certainly not in three days, something that should go down to the high school teacher. What we should do, if we think it worthwhile, is to set up machinery to make this sort of thing a continuing process. Right now it's an event of three days' duration. Is there a process we can set up so that this type of thing will, in due time, become effective? Can we set up an operation from which these other lower levels can constantly feed?

An operation of this kind would have sense only if we could, perhaps, arrive at a more concrete picture of what the concepts are, anyhow. What are we talking about when we speak of concepts? We talk about terms, we talk about boxes, we talk about disciplines; this is a hodgepodge of completely incommensurable things. I'd like to see this group go through the experience of trying to formulate what each of us thinks is a concept. How would it be if, tomorrow morning, we would pair up—I think it would work better if two people could get together—and spell out five basic concepts as samples? Throw these things into the hopper in the afternoon and, if this hodgepodge assumes any shape, one could perhaps farm it out and have follow-up conferences on these individual things in due time, or bring it back to the Council.

[Several modifications and counter suggestions were elicited by the foregoing proposal, and some discussion of what should be done immediately versus possible postconference activities. Attention then moved to a consideration of what is meant by the term "concept."]

WEISS: I think there has been too much name-calling in this whole business, where people apply names without quite identifying the content; I have been much more concerned with trying to bring out again, the factual content that has been submerged under such names. Take the concept of "normality," of "specificity," etc., being used all the time in biological fields. Is there a concept of specificity? Perhaps somebody could try to spell it out from his one-sided view, and then we let the people from other fields have a look at it and say whether it agrees or doesn't agree with the connotations in their field, as an exercise to see whether there is really any bone of contention at all—maybe there isn't. I still am not quite convinced from the turn of events here this morning whether very much can be done by group action and I'd like to see this acted out in front of me.

GERARD: I am suggesting that, before we write a book we have a table of contents, and then we can assign chapters. This is the only degree of structuring that I would like to have.

BRINK: I would like to ask a question. Is the term "concept" being used here differently from the notion of operational, operationally defined, concept? If you mean the latter, which all scientists are striving toward all the time, the list is sort of endless. What one does is define a concept by some sort of improvement in experimental or theoretical procedure—you polish it in that way. If you want to get a general set of concepts under one term all you can do is to list them and say, "This is the list that I mean by this more general term." But each item of the list should have operational definitions.

WEISS: What Gerard means is whether, if you set up a whole set of these concepts, you could set up some degree of hierarchical or other logical order that will interconnect them. Isn't that what we are talking about? Are there any all-embracing things which would subsume, as the nice term is, several subordinate categories?

REYNOLDS: I think the term "conceptual framework" is the term I first heard applied

to this, which is a little different from a string of pearls.

BRINK: If you are thinking of concept as somehow or other opposed to empiricism, then I'm puzzled.

WEISS: I don't think anybody is.

MILLER: Is it helpful to be concrete? Here are six concepts: cephalization, hormones, evolution, species, organization, sperm. I heard all those mentioned this morning. Now which kind of concept do we want to define? I'm not name-calling, but if we all went off and listed concepts we'd come back with very different sorts. We'd have to agree, I think, as to what sort of. . . .

MAYR: In a way I like better "generalization," because the word "concept" is too much like "term," and some of the "terms" mentioned by you are not generalizations at all. Perhaps it would be safer to use "generalization."

WEISS: Maybe we shouldn't talk about concepts at all, but about the procedures that go into the making or breaking of concepts; I mean what sort of precautions to observe and yet be successful in formulating and dealing with concepts. Aren't we talking about dealing with concepts rather than talking about concepts?

MAYR: How to arrive at generalizations is certainly one of the things we should study and discuss. Yet, I don't see how we can neglect the generalizations themselves. Incidentally, I think Miller's list of "concepts" is very instructive because it illuminates the danger of using the term "concept."

GODDARD: Actually, when we started this as a conceptual framework of biology we were certainly thinking of what are the major generalizations. But by no means did any of us believe, in the original planning, that we were going to try to get away from the empiricism of science. After all, generalizations which turn out to be empirically unjustified and unverifiable are just not good generalizations. The breaking of knowledge into fragments is useful for one part of scientific thinking, to make you look at these things, but if this is all that you have it is merely a lot of isolated empirical observations or a lot of isolated symbols for them.

MAYR: Does the word "generalization" indicate to you the inductive method?

GERARD: To me, yes, exactly.

MAYR: Which I think is very important.

BRINK: It automatically does if you say that in generalizations there should appear no terms that are not operationally defined.

MILLER: Aren't we looking for a statement which could be put in the form of a simple sentence, actually, which we believe applies to more than one biological phenomenon? In other words, a single word isn't what we mean, but rather a statement.

HOTCHKISS: We don't want a noun, we want a declarative sentence.

[The chairman here pointed out that the discussion had drifted from consideration of objectives to detailed views on the role of generalizations. He underscored the suggestion that if the conference was able to produce something worthwhile, machinery to keep it going should be set up. He then asked for further views on objectives.]

REYNOLDS: May I make a suggestion? I wonder if one of the things that might be most useful would be, let's say, a model of the field of biology, undoubtedly to be bolstered up and contributed to by generalizations, that would facilitate looking at it. One of my motives and interests in this was that I recently discovered for myself a new system of coordinates or boxes, whichever they are, for the utility of biology. While I think the utilization side of biology may have entirely different attributes from the fundamental, is this not the kind of thing that needs to be done elsewhere?

I think it was Gene Landis, some time ago, who published a little chart consisting of three or four overlapping ellipses which tended to show relationships between fields in physiology. This was not without some inconsistencies, but it gave me quite a few ideas in terms of relationships between subfields that I hadn't seen before. In other words, it was a visual model of his way of looking at this field. And I wonder if that kind of thing would be helpful, more broadly.

WEISS: My feeling is that we will not arrive at a redistribution of biological disciplines in three days and, if we did, it probably would have no effect. I feel, however, very much as Mayr does, that by

concretely spelling out at least some of the things we ought in the future to deal with, we might set a trend going.

What I'd like to do in the future is: (*a*) prevent people from continuing in an empirical and unscientific way, coining whatever concepts they please and then operating with them irrespective of what goes on in neighboring fields; and (*b*) alter the situation where those who do concern themselves with a conceptual order across the board in biology are the ones who have little practical familiarity with biologic phenomena and deal merely in abstractions. Something can be accomplished in the direction of these two objectives only by creating healthier habits of thought, starting from the educational level up; for a major improvement we have probably to look to the next generation.

Now, I think there is not going to emerge any universal scheme, except one so general that it is tautological and trivial. Naturally, we can't do much about this. But we can demand at least that those various principles that are being used be applied with a certain sense of scientific responsibility. How do you do that? I don't think you do it by a single conference, but perhaps by having a group of conferences, coming to grips with a few simple exercises in conceptual clarification. Let the rest then take a lesson from that and develop the process into a standing habit.

GERARD: Are you thinking that after this conference is over we may have somehow or other gotten several smaller areas which could be worked on further by subgroups, or by the whole group at another time?

WEISS: I suppose so. We have, for instance, five area committees—for molecular, developmental, genetic, regulatory, and environmental biology. Each one could concern itself with conceptual clarification within its own field.

MAYR: I feel that on the third day of this conference we would be better prepared to say how best to continue than we are right now, because we don't yet know quite what's going to happen.

GERARD: We are also trying to have what happens fit into a possible use.

SCHMITT: I might relate an experience which bears on this. This year there were some 75 high school teachers, picked from all around the country, who were brought to M.I.T. under Westinghouse fellowships. They were given lectures by at least four departments—our department gave 14 of them, I think. I think something like 20 of the 75 were teachers of biology. In my lecture on basic matters of cell physiology, I suggested that we start out with the assumption that, since many of their students would have had no chemistry or physics, we would not use chemical formulae of any kind but would see if we could arrive at any concepts about cell physiology without benefit of detailed chemistry. We undertook this and afterwards a number of these teachers came around and said, "Is this kind of thing written up anywhere? We would like it as a text."

From what these carefully selected high school teachers said about the textbooks they now use the situation is appalling.

At least one teacher thought it would be a useful thing to have a bulletin which might be used as an addendum or help to whatever textbook they now use—something, so to speak, crystallized and from the horse's mouth, in this area. The bulletin, perhaps several of them, would be a significant segment of what any of us would feel a high school student should know something about; the kind of thing that is likely to be a challenge to a student. Where is the glamour in modern biology for high school students? Apparently, in the popular mind, biology is not the basis of investigation of disease, it isn't the basis for some kind of a rational way of living for each of us. Rather, it is the impression left with them by a brief brush with the subject in high school.

SPUHLER: I was wondering if we were all in agreement about what we mean by biology. It seems that it's been used in at least two different senses: (1) the general rubric which will include all the special objects; and (2) a general science, a concept of biology that would have to apply across the board. Is that what you mean.

WEISS: In the Biology Council, "biology" is definitely a collective term for all the life sciences. We are not talking here about the general principles of biology—teaching a

general course—but simply concepts in the life sciences.

SPUHLER: Well, all of these lists of six, then, are concepts in your sense; in the other sense they wouldn't be.

MILLER: Why don't we figure out, not boxes, but lectures which would cover the field, and perhaps think who would best give these lectures. Then have these lectures given, not necessarily all at the same place, recorded, and transcribed, finally to be reviewed by a panel. There could be certain ground rules, such as: "Don't have any complex chemical equations;" "Don't have any complex mathematics." Another might be.... (GERARD: Don't have any complex organisms!) .... insistence on cross-species comparisons, and so on. This procedure might give you a document which had more integration than most symposia do and have more coverage than any individual can accomplish.

WEISS: I am a bit worried about this, Miller. I think this might be similar to the current trend to divorce education from subject matter or to separate administration from subject matter. We might try to distill concepts off in a sort of factual vacuum. Some of the so-called theoretical biologists are doing this. I have a feeling it is much easier to deal with these concepts by applying them in rigorous form to any one particular specialized field, repeating the performance—you in psychiatry, I in development, he in physiology and biochemistry. By being rigorous in all these performances, a sense of the rigor of concepts will gradually develop, automatically. I think this is probably a better procedure than separating the concepts off.

MAYR: The attempt to distill off abstractions and to separate them from their factual basis has produced disastrous results in history teaching, particularly at the elementary level. One cannot discuss intelligibly the institution of monarchy without first studying the reigns of a number of monarchs. Likewise one cannot study biological principles, unless one has first studied animals, plants, and other concrete biological phenomena.

BRINK: A procedural suggestion, based on this conversation, is that we break up our considerations into two classes, those which deal with science as we scientists practice it and those which deal with what I might call pedagogical shading of scientific fact to interest the student or reach his level of intelligence. I think that if we mix these together we are going to defeat our objectives right off.

GERARD: May I direct our attention back to objectives, as a whole? We have gone off on a discussion of one objective that was suggested, namely, to improve education in biology. Are there any other objectives for us?

REYNOLDS: I would like to advance again the idea of a simplified structural framework for the whole thing. Here I didn't mean that we were going to abolish existing disciplines or convince anybody of anything along these lines, but I have a feeling that one of our worst troubles is that we have such a hodgepodge of content there that it is very difficult for anybody to look at it all at once. I feel that we should try to present one picture, or at least a finite number of pictures, that people can look at.

For example, if we were to adopt the idea of these lectures, the very first thing that we would have to do, if they were really to cover the subject matter, would be to decide how we were going to compartmentalize it, logically.

GERARD: You are all affirming that the only way to begin is with something. If we begin with nothing then we must go through the process of creating something, which is a tedious and difficult job in a group. So I again suggest that we start with something already before us, from which we can remove or add.

Whatever our total objectives, certain things may deserve more emphasis, or less, in the limited time we have, depending upon what our main hope is. If this is to achieve some community of understanding in this particular group so that each can later do individually some sector of a larger task, that is one thing; if some collective output from this group is expected, this is quite a different thing. An attempt to isolate important generalizations in biology, to cut

across wide boundaries and present a list which any teacher could exemplify, is different from one intended to sharpen certain of the concepts and rid them of overlapping words, or make clear how the whole field of biology can best be divided administratively, as in organizing a new university. We might want to do all these things in different degrees, depending on our aims, but what can we realistically hope to achieve? Which areas are the most important?

WEISS: If you want some simple, concrete task, why not just hit the most overriding of all the principles that we are constantly mouthing? I would really like to know what every member of this group makes of the concept of organization. We speak of organisms and organization all the time. What does it mean? Do we all agree that this is a concept?—(MAYR: Is it a generalization?)—Frank Schmitt spoke of organized enzymes this morning, somebody speaks of organized matter, and somebody else of organized behavior. The generalization of all this is something we distill off and call organization Now, what is it? I'd like to be given a sample.

GERARD: Here is a concrete suggestion: we cease consideration of what we should consider and begin to consider something. Is there an alternative proposal for immediate procedure?

BRINK: Did I understand you to say, after I spoke just before, that our objective is the teaching of biology?

GERARD: No, I said the discussion had been in that direction.

BRINK: Then are we continuing that now, or are we now considering this organization idea rigorously, rather than relaxing it to a certain level?

GERARD: I am trying to set the level of discussion. We agreed that, before setting up a procedure, we would look at the desired outcomes. It was suggested in that connection that we wanted to improve teaching and that one way of doing this was to identify some of the important generalizations in biology that might be useful to teachers at different levels. The question then arose whether we should set up a continuing process to insure the further development of conceptual analysis. We entered a number of particular paths here and discussed, among other questions, whether concepts are precisely formed by thinking about them, by defining operational terms, or by some other approach. I then asked that we again look at the whole list of possible outcomes; which elicited nothing more at the time.

Now, if we are ready to focus on some particular thing, let's just barge in.

[During the reel change, which occurred at this point, Dr. Caspari brought up the question whether there were differences between biological and non-biological concepts, and if so, what those differences were.]

GERARD: Are there any other suggestions?

HOTCHKISS: I think Dr. Brink made one that was put in the category of procedure a little too soon. He asked, "Are we going to consider things from the standpoint of the professional and the student separately?" That's a question of objectives.

I was thinking of another thing. Earlier I felt a little more optimistic than I do now that we had a good chance of bypassing the rather difficult problem of ordering knowledge. I didn't feel that we would really differ about the relation of pharmacology to physiology or to biochemistry. But what we might do in a short time is try to solve why it is that a pharmacologist uses a physiologist's equipment and somehow thinks he is doing a different kind of thing, and why a biochemist thinks the pharmacologist is missing the point. What are these obstacles between the people who are working in the field, not what are the relations to the concept?

GERARD: We have two suggestions: (1) start talking about the concept of organization; (2) consider any critical differences between concepts of biology and concepts in the physical sciences. How would you phrase your proposal?

HOTCHKISS: I would say, concretely, to outline these general propositions, not in a comprehensive attempt to order all existing knowledge, but to inquire into the differences in point of view which make it so

difficult for a person of one biological discipline to communicate well with another, right inside biology.

BRINK: It seems to me that if we did what Dr. Caspari suggested, we would be doing just that—(MAYR: I feel the same way.)—to put it generally, if one uses physical and chemical instruments to make measurements on biological objects, in what sense are we finding out something different?

GERARD: Are there still alternate suggestions for immediate procedure?

MILLER: If we deal with organization as a specific example of a problem, I would hope first that we dealt with it at a high level of scientific conceptualization and put off until later, if at all, the discussion of how you shape it for educational purposes.

[Dr. Gerard recognized a fairly strong sentiment in favor of considering what is unique to biology, and called first upon Dr. Schmitt for comment.]

SCHMITT: Well, Gerard, you were at Ann Arbor where we met with a group from the midwest to consider what they thought biophysics is. The group there, as it turned out, included the entire spectrum from physicist to physiologist, biologist, and so on. Some of the more physically minded ones had rather naive conceptions of biology, but they were attempting to make measurements of biological properties more precise.

Biochemists, by and large, are differentiated from organic chemists in the degree of complexity of the materials with which they work. These are people working largely on problems of biosynthesis. They use partial systems, derived primarily from cells; that is, they are able to get enzymes out and, using them on the right substrates and in the right sequences can cause something to be synthesized. The inference is that similar reactions occur in cells. This has provided us with a very powerful point of attack concerning the organization of the cell, although the isolates may behave differently within the cell than they do *in vitro*.

Biochemists have, perforce, to use organized partial systems because one can't, except in a very elementary way, accomplish much synthesis *in vitro* without the aid of organized bits of cellular material. To what extent is the chemist interested in such matters different from the biologist? Primarily, I think, in the extent to which he is cognizant of, and takes into consideration in his theories, the organization of the cell and of the interacting subunits in the cell.

By organization I do not mean only that which can be seen with a microscope; I mean the organization right down to and including the atomic distributions within the molecules of proteins, lipids, carbohydrates, and so on. I referred before to organized patterns of enzymes which occur in the laminar, two-dimensional systems in mitochondria built primarily of bimolecular leaflets of lipids. Apparently such systems provide the two-dimensional structure—the "floor space" on which substrate molecules may be passed from one enzyme to the other—and in this way bring about very efficient, sequentially ordered biochemical processes.

In the production of the biologically very important fibrous macromolecular particles it appears that relatively simple units (protein molecules, of 20,000–30,000 weight) are coupled end-to-end in a special manner so as to provide very asymmetric particles which have different and new properties than the smaller molecules of which the macromolecules were built. The difference lies in the interrelationships or organization of the subunits. Some of these properties are not easily explained by current physical chemical concepts. These properties, I am convinced, are the basis of contractility wherever it occurs (muscle, cell division, etc.), for the mechanical properties of fibrous systems such as connective tissue, and for chromosome interaction.

This is an example of a very complex biological enzymatic mechanism by which proteins, nucleic acids, etc., themselves possessing highly specific structure, can be built up into still higher orders of complexity from which emerge properties which are not obviously inherent in the simpler components.

GERARD: You have not given a criterion by which I could separate biology, as a group

of phenomena, from physical chemistry on one side and sociology on the other. Your statement is valid each time one goes from any set of units, however complex, to a larger unit.

SCHMITT: The best example would be chromosomes. Here you have macromolecules which, when ordered in a certain linear and lateral pattern, are capable not only of influencing the chemical and structural processes of the organisms, but are also able to bring about their own biosynthesis; the whole process is adaptively ordered to produce the final organism.

GERARD: But is there any difference in kind? Of course, you can draw the line and say, "At this point I call it biological, at this point I don't." But is the problem of the elements and their organization in the larger system inherently a different one when dealing with enzyme molecules in a mitochondrion or in a chromosome, and when dealing with electrons in different shells of an atom?

SCHMITT: No, I would make no distinction there. This would differentiate me, I suppose, from another type of biologist who would feel that you've got to endow this stuff with special vital properties other than those we know.

[Dr. Caspari and Dr. Schmitt debated briefly the implications of the latter's use of the word "efficient" in referring to enzyme activity within the cell.]

CASPARI: There is really a quite fundamental difference in the way you and I are looking at things. The way in which you describe the thing means always building it up into a more complex system, but in the complete description of a process of this kind there is also the reference, which we call functional, to the cell. The term "efficient"—and that is why I was asking—means more efficient from the point of view of giving energy for the cell. So the biological process has to be described from both sides, and that also applies to your chromosome. You can ask very definitely the question, "What is the function of a chromosome, or of the locus of a chromosome, in the metabolism of the cell?"

The proposition that I want to make is that a characteristic of biological organization is that you can look at it from two sides. One, it is built up of smaller units and as these smaller units are put together new properties are formed. On the other hand, I think we must look at them from the other point of view—from the higher level of integration—and ask ourselves what was important for the higher level of integration. I am really asking, "Is this a legitimate question or is it not?"

GERARD: Paraphrasing a bit, are you saying that, whereas in sub-biological systems one is ordinarily satisfied to ask the question in terms of causality, in biological systems one is likely to turn it around, and ask in terms of purpose?

WEISS: I'd like to come in here because I think I can pinpoint this question in two ways. In the first place, if you have n enzyme molecules present in a substrate, the efficiency of those molecules in kinetic terms would be quite different if they swim around in thermal agitation, as against being fixed in an organized absorption where a much higher concentration per unit of surface is exposed to substrate. Therefore, I think it quite invalid, as is often done, to deduce enzyme concentration from enzyme activity of the organized living cell; it tells you the efficiency of the process and that is not simply a matter of total concentration.

I think there is a fundamental difference to which you refer with this word function, which is one of those weasel words in biology. Let me see if this term "function" cannot be identified operationally. Let me try this out on you.

If the biochemist studies a process in isolation, *in vitro*, he has to provide superabundance of the elementary components, optimal conditions of pH, temperature, pressure, and what not. The cell normally does not provide optimal conditions ubiquitously for all things that are going on in the cell. Conditions for each particular reaction are localized; in any other place in the cell, conditions not being optimal, that particular reaction would not take place, hence we never see the particular compound;

it has no probability of existence. I place it purely in terms of probability.

Now, in order to get the conditions for this given process to occur in a localized part of the cell you must have, according to some of the older biologists, a physical framework of some kind, a micro-skeleton separating [the cell] into little chemical laboratories with different chemical properties. This we now know is not true, because you can stir protoplasm up and it will put its content again into correct distribution. So you don't destroy organization by destroying the visible expressions of organization.

What is it, then? Well, obviously, you have numerous such processes going on, all very subtle and fussy in their requirements. Let's take just two of them. If these two processes go on in what we call an organized cell, side by side, the conditions of their operation must be mutually compatible. One reaction must be producing something which the other can use, or at least which is not incompatible with the continuation of the neighboring reactions; you must have a sort of symbiotic relationship between the two types of chemical systems, so that their substance and energy liberation or consumption, and so on, are in a sort of resonance.

Thus you can set up systems, as we have tried to do, which are mutually supporting each other, one creating the conditions under which the other one can go on. You can figure this out for two reactions, but you cannot possibly do this for more than two. You are up against conceptual problems, very much like the three-body problem in physics, if you have three such interdependent reactions—I don't call it functional relation, I call it interdependency—so that any two are necessary for the third. I think the resultant system is something you cannot, therefore, build up by actual synthesis, but something which has stability only if the components are present all at the same time.

If you extrapolate from this picture now, you'll find that in the living system you have to have a lot of these interdependent systems present at the same time, otherwise you have merely a surviving, but not a living cell. You can isolate some components—elements of contractility, mitochondria, units of respiration, and so—but you'll never make a living cell from assembling them one after another; you have only parts of the thing surviving, which you were lucky enough to isolate. Now, I think organization consists essentially of just this interdependency of component reactions, rather than of any fixed framework.

GERARD: We now have all three dimensions suggested: Schmitt thinks of organization in terms of building the complex from the simple, a causal approach; Caspari, in terms of the role of the subordinate unit in the whole, a functional or purposive approach (I still think you smuggle purpose in, but this is all right); and Weiss, in terms of a timeless pattern of relations or interactions between certain complex entities.

[Attention here turned to what is meant by the word "function," and the following views were expressed, not necessarily in the order given below.]

CASPARI: I think Gerard is quite correct in stating that the term "function" may have something purposive in it. The question is whether this is something we want explicitly to legitimatize or not.

WEISS: I used to use the term in physiology for those properties which the observer assigns to the operational activities of the system because he knows it's necessary for the survival of the system, rather than something you can derive inherently from the measurable properties of the system and its components. As it is used in engineering, I think it ought to mean the role or utility of the system with respect to the next larger system of which it forms a part.

MILLER: "Function," in biology, often is used in the same way as "action," in physics. Then there is the word "function" as used in mathematics, which is essentially a characteristic of a variable. It might be desirable to ask, "Do we by 'function' in biology mean anything more than you do by 'action' in physics?" The trouble with purpose, it seems to me, is that it has a teleological implication and, while there are other uses of the word "purpose," whenever

you use "purpose" you think of it teleologically, which is a very dangerous concept.

BRINK: This can be given operational meaning. Certain properties that are essential for fitting a given unit as part of a larger entity can be operationally defined as functional properties. You can go on and describe exactly what you do in order to identify the functional property of a cell.

GODDARD: I think, very frequently, the biologist unfortunately includes the result of an action, as well as the action, in function. You could define the function of a contractile vacuole entirely in operational terms, without trying to work in anything else, but I am afraid the average biologist includes the concept that the vacuole regulates the size of the protozoan.

BRINK: This is a recommendation, that functional properties always be defined operationally.

[Discussion returned to the problem of "organization."]

SPUHLER: I wonder if the simplest type of organization requires [only] that you recognize a system with at least two parts that have relation. For instance, we say that a flock of hens has social organization where there are at least two hens and there is an order of dominance. Would not all statements about organization be hierarchical with regard to structure, operational for equilibrium, and existential for history? In the very simplest systems, you have only two parts in some relation, but don't you have an organization?

WEISS: No, I don't think this is organization—it is only heterogeneity of composition, like oil and water. To have heterogeneous parts is a necessary condition for organization, but it isn't sufficient.

WRIGHT: If you could show that this activity contributes to the persistence, to the self-regulatory properties, of the flock, then it would be an element of organization.

WEISS: But then you don't consider just the two hens, you consider the flock.

[The Chairman at this point summarized the immediately foregoing discussion by pointing out that a search for the unique attributes of biology had led to the explicit question of organization. An informal vote favored returning to the more general question, "What are the uniquenesses of biological principles?"]

HOTCHKISS: I very clearly felt that we should vote as we did, yet the thing I most want to say pertains directly to organization. At one time I tried to define living matter and came out with a definition of about four key words, "Life is the repetitive production of ordered heterogeneity." Heterogeneity isn't enough. When you recognize relationships in the heterogeneity—the order of it—you begin to talk about another aspect. In the order you may imply function. "Production" has to do with the biochemistry, usually, the whole realm of biosynthesis. The repetitiveness of it has to do with the genetics part.

Order is usually perceived by another ordered observer. All the way back to the optical isomers of tartaric acid, Pasteur himself was the optically active thing which could selectively pick up the different crystals of tartaric acid. It was either the observer, who was a highly asymmetric organism, or it may be an alkaloid or something like this with asymmetry. This is usually the case with all our ways of discerning order; the biologist is looking at things as a very highly ordered observer, but often only interested in certain aspects of the order he witnesses.

Here's an organized thing [displaying key]—we call it a key and say it will turn a specific lock. The observer may describe it: "This is a series of notches on a brass bar." This statement seems complete to an analytical chemist. When the physicist observer adds that there are two low notches and three high ones, or so many medium-sized notches, etc., he is still only talking about statistical matters. It may seem to be a description of non-randomness, therefore order. But only when he says, "This notch is next to that one, and this other next to that," he begins to describe the order that is significant to the lock. I think this is parallel to the meeting ground of the physicist and the biologist in biology. The "order" that seems meaningful to the physicist or chemist does not yet necessarily fully describe those aspects of the order which a biologically

specific observer, e.g., another molecule, cares about.

GERARD: How badly do I distort you, in summarizing what you say in this single sentence: "An area of interest is at the level of biology when the observer has to be highly polarized, or asymmetric, in order to make the required observation?" This is a very interesting statement.

HOTCHKISS: This, although only a minor facet of a big issue, is my point.

WEISS: But isn't there a basic difference in the fact that the serration of that key is not a property of brass but of the man who made the key? That is, it's a property of an organism, whereas in the natural systems which we are studying, these things are inherent.

GODDARD: Actually, I can add very little to what Hotchkiss said in his simple definition. The one thing that seems to me unique about these systems is the fact that they do not arise *de novo*, that one form, one organism, comes from a pre-existing form. But I think we also wish to recognize that this is possible only under certain environmental limits, although the materials taken from the environment can be essentially unordered and can be very dissimilar to the final structure that arises. The environment need not always furnish highly ordered materials or materials of very similar chemical composition. The simplest case is the green plant cell, requiring nothing beyond inorganic salts, CO_2, and light—no amino acid, no vitamins, need be in the environment. But you must start with one cell; you can't start with less. From here, I think, biology has to begin to operate, empirically and operationally; I know of no systems in physics where the same sort of repetitive reproduction occurs.

MILLER: I've been wondering just who was going to be the first person to use the word "information" in this discussion and it looks like I'm going to be. Some of the things that have been said have a good deal to do with information theory. Order, organization, and form all are related concepts. Some who have thought about cybernetics have seen in feedback mechanisms, which are obviously of great importance biologically, an aspect of information theory relatively less important than certain others.

A more fundamental notion is—and I have to be philosophical—something like what Plato and Aristotle were getting at when they talked about form and matter. I would suggest that the work Quastler is doing in biology is, perhaps, going to be the most fundamental approach to the problem of organization. Not only the counting of bits of information, but the ordering of the various types and developing other dimensions of information may be getting at the most fundamental ways to deal with organization at each of the levels that we are interested in. What we call qualities are aspects which can be quantified by informational units in the same way that quantities can be dealt with in terms of the ordinary measurement units that we are accustomed to using, and that a complete description of any system, whether it be a cell or an organism, requires a quantification in terms of energy present and formal structure—which latter, I think, will be done in organizational informational units. And this basic aspect of information is not widely recognized, because it is really quite different from cybernetics.

SCHMITT: I submit that this may be mostly formal! What would one say about "bits" of information in nucleic acid, if he didn't have a double helix to work with, and how could he know about double helices five years ago? One could have talked about bits of information assumed to be on nucleic acid or on a chromosome in a general way, but whether you would get more out of it than you put in is questionable, except again in a formal sense.

When you've got something concrete to work with it becomes very interesting and, I think, very productive. But behind all this is a realistic, sometimes stochastic, dealing with the structure of organized materials. And the thing we are addressing ourselves to is, "To what extent is there anything different in biological systems?"

In embryological problems, for example, one deals with such things as morphogenetic fields which have to be explained somehow in physical and chemical as well as in biological terms. Once the phenomenon is

discovered it can then be defined and analyzed. But how much must be known and what concepts formulated before such fields could be predicted to exist?

Were any of you at the meeting about ten years ago, in Washington, of theoretical physicists? They chose to discuss the question, "What is life?" Most of the participants were physicists, but a few biologists, such as Delbrück and Muller, were relied upon to outline the problems which the physicists were to explain. Particularly emphasized was the question, how are the chromosomes oriented, vis-à-vis each other, so as to induce homologous pairing and other processes of meiosis or the "field" nature of such processes.

WEISS: An electric field?

SCHMITT: Yes, not just in the sense of formal charges, but total electric field.

WRIGHT: Not just a field, but a field that has the property of bringing about duplication of pattern and with the added property, if something goes wrong, of duplication as of the new sort. Once you've got these two things, you've got, it seems to me, the basis of life. And that's got to precede your three-body problem. The three bodies are not going to get together until you've got patterns that have this duplicating property, no matter where they draw their energy from—they don't need to be autonomous in energy.

WEISS: What do you mean? By themselves?

WRIGHT: They can draw their energy from outside; I don't care about where that comes from. [All that is needed is] just a pattern, which somehow or other can multiply itself in the right environment. It's got to duplicate itself as of the new sort, if something goes wrong—it's got to have hereditary mutation. Then you've got the basis for evolution. As soon as you've got that capacity, you can build up your three bodies, perhaps not immediately, but get two bodies, ultimately three. But you haven't a reasonable chance of getting these interdependent systems until you've got this basic property.

SCHMITT: What I was trying to get across was that you have got to look to the complex system, to have the phenomenon presented to you, in order to analyze it; you cannot deduce the existence of the more complex, else why has theoretical biology been so slow in coming?

GERARD: Much of what you said might be put this way. The unique thing about biology is that it must ask the kind of questions which have to be answered by these other procedures.

SCHMITT: Well, it deals with the complex material itself, rather than with the simple materials, and it discovers those complexities—which then have to be explained in the simpler sciences. If the embryologists hadn't made these transplants and found their potentialities, we would never have known of this field problem.

REYNOLDS: I've been concerned by the fact that purposefulness came into our discussion so many times. For example, in the key analogy, it seemed that the purpose of this in turning a lock was predominant in the biological view. In the light of what Schmitt said, it seems to me you might define living matter as the only kind of matter which initiates activity.

WRIGHT: There is a lot going on in the sun, which presumably wasn't initiated by living matter.

REYNOLDS: True.

MAYR: Well, at one time somebody defined the differences between living organisms and physical phenomena by saying that every living thing carries its entire history with it and could be understood only in terms of its history. This is certainly not true for most physical and chemical phenomena. A sodium atom has definite properties, regardless of whether it was previously in a salt or an acid. But while the separate processes of a given species or individual can be analyzed in chemical and physical terms—the particular way in which they are put together and give viability to the whole is something that, in the final analysis, is understandable only in terms of the evolutionary history of the particular line. This may not differ in principle from physics but it certainly does for all practical purposes.

A second difference that I can see—possibly not a complete difference, if you think of Pauli's individual gases—is the phe-

nomenon of populations. At certain levels of integration in biology we have population phenomena which can be described only in statistical terms, but not, as are most physical phenomena, in any typological way.

And a third difference, again as seen from my particular branch of biology, is the problem of individuality, such that no two individuals, no two species, are the same. Such complete individuality is, on the whole, absent from the physical world, which often makes it very difficult when a physicist and a biologist get together. The physicist, unfamiliar with this type of thing, has great difficulty in understanding it.

Now, in all these things that I point out to you, there may not be a basic difference of principles; there may be an area of integration. I suspect that the macromolecular level of Schmitt may be that very area, but there certainly is, for the practical work of the biologist, a difference from the field of physics as I see it.

GERARD: You are now offering three other criteria, besides organization, which we've been discussing, for separating biological phenomena from others.

WRIGHT: I think this property of duplication as of the new sort is the thing that makes possible this individuality and history that you spoke of. If you've got a complex thing which, whatever the source of energy, can impose its pattern and which has the additional property that, if something goes wrong and the pattern comes out a little bit different, the same mechanism brings about a duplication of the new sort, then you get elaboration of complexity. You're bound to get individuality, and history is inevitably carried along. These are just consequences of this more basic property.

MAYR: One is a mechanism, and one the result of that mechanism.

MILLER: It would be a little better not to ask, "What is the difference, if any, between biological and physical phenomena?" but—"What are the special characteristics of biological phenomena that are not seen in *other* physical phenomena?"

MAYR: That is a perfectly acceptable way of putting it.

MILLER: We are asking a question which is as illogical as asking what is the characteristic of this genus which is not found in the phylum of which the genus is a member. And that's not a sensible question. Rather, you are saying, what are the specific characteristics of this genus that aren't found in any of the other genera of the phylum?

BRINK: Is there a way to formulate a set of concepts comparable to a formalism such as thermodynamics, but including additional terms which cover all physical and chemical *and* biological systems, by whatever criteria you use to recognize them? If, then, certain terms tend to a very low value you have what we call isolated physical systems without biological properties. Now if such formalism were possible, it seems to me it would give you all the advantages that accrue from formal thermodynamics to physics. And I don't quite get the point that Schmitt made about this formalism in relationship to particular knowledge, because it is true throughout physics that the formalism of thermodynamics has precise physical meaning only after you have done some sort of an experiment to determine the processes you are talking about.

SCHMITT: Thermodynamics is statistical, and what we were talking about was bits of information on a very particular kind of array.

BRINK: Yes—but this would be the extension of the formalism that I'm talking about. Some years ago it was found advantageous to give certain molecular systems a property called internal energy—it was designed to do a certain job. It was decided that entropy was an important property to impose on a system in order to systematize what you were talking about. But before you could make any use of this kind of formalism in a particular instance, you have to do some experiments. Now, I would carry this picture along one step further—invoking information theory, but let's leave out the bits—to say that it may be a way of doing it. Information theory, in a more general sense, may be just another formal operation that we want to impose on molecular systems which will take into account the

kind of [phenomena encountered in biology].

SCHMITT: No objections. I might, however, just extend this a little bit, since you've brought in thermodynamics. Now, we don't have to retain conservation of matter as a doctrine any more. Under certain conditions even energy may not be conserved although I think we still have to believe in the conservation of momentum. Now, is there anything about the life sciences which may eventually require any basic physical concepts to be changed?

MAYR: Whenever that question comes up between biologists and physicists, it is curiously enough the physicists who think that maybe some of their concepts may have to be changed.

SCHMITT: That is correct.

MAYR: Biologists, invariably, feel everything can fit very well into. . . .

WEISS: Doesn't that just emphasize what Miller said—that you can't set biological systems in opposition to physical systems. After all, they must comply with these. The question is only, have you got to add something when you get on to the biological systems.

GODDARD: We are asking, "What are the minimum number of elements that we have to add to the non-living physical systems to have the minimum living system?" Now, we've had a few of these already given to us; I like the way Brink has handled it. We recognize, at the present time, that we have to add on enough of an ordered system, to bring in an adequate amount of information, so that the pattern of that ordered system can be reproduced from the environment in which it is present. Now, how big, how much, does this have to be? I don't know. Of course, in the simple cell it is still a very large number of bits of information.

SCHMITT: That's highly formal.

GODDARD: All right, I would only say this, Schmitt, we're asking what were the minimum number, for example, of your macromolecules, and what particular arrangement of these macromolecules is the minimum system to be put together, that will do this job? I don't think that we are talking "anti" your macromolecules at all.

SCHMITT: I can't see that you are getting anything more out than you are putting in. When you speak of information, the information is in a code, isn't it? If you succeed in decoding any of it you will be essentially making discoveries in one or another aspect of biology—genetics, physiology, and so on.

GODDARD: Can I answer this one, Gerard? I would say coded information is your ordered system.

BRINK: It's very common, in physics, to look at a system from more than one point of view, to use the information from the two conceptual schemes. This would, for example, combine a detailed molecular viewpoint with some sort of a statistical average, to come up with a thermodynamic quantity which you would then use in discussing a macroscopic property of the total system. There is nothing illogical in mixing formalism with a detailed molecular mechanical viewpoint.

SCHMITT: I have no quarrel with that.

BRINK: The only question then is, "What do we have to add at the formal level in speaking of organized molecular systems what kind of an operationally defined quantity which, for a particularly well-ordered system, we could perform a set of measurements and give a value?" That set of measurements probably would be just what you are trying to do experimentally.

MILLER: Mathematicians here will understand this better than I do, but I understand that Birkhoff, a few years ago, working on aesthetic measure first developed statements mathematically, in an exhaustive way, for the least information that you need to describe the whole realm of objects, that is, all the forms that there can be. With a certain number of bits of information, you should be able exhaustively to describe all forms for all organizations. Then we are able to have a complete description of how an organization can occur.

I don't understand all about the tactoidal forms Schmitt mentioned, but he says there are 18 of them, and that's all there are—that exhausts the possibilities? This is very close to what Birkhoff was doing for all forms. We can recognize that there can be a finite number of bits of information which can convey all the types of form that there are. The

question is, are the chromosomes conveying this information, or is any other reproductive system conveying the information?

GERARD: Brink, are we getting near your operational demand?

BRINK: Well, I was wondering just now whether, in solving any problem, you really do use these as alternative approaches. It seems to me, you use them simultaneously.

I certainly didn't have in mind any sort of abstract consideration that hadn't been a result of generalizations built up initially by a series of instances. You've found out, for example, that entropy and energy of a set of molecules was a useful thing to have, and you had an operational way of getting a value for it. I guess we are really saying, "We want some operational procedure for evaluating relationships—a molecule can't be characterized just by describing its structure, its internal energy, its entropy content, but must be evaluated with respect to some receiver that's going to recognize it and do something that it wouldn't if the first molecule weren't present."

GERARD: Mayr, this was a rapid descent from your level of discourse, through Wright's statement that individuality, population, and history-bearing are simply the consequences of a self-perpetuating system, back to the level of the molecular-biological and the formal mathematical model. Are you content with this, or do you still want discussion at the level you were using?

MAYR: Well, after Wright brought out this important unifying point, it occurred to me that individuality in a majority of organisms is due not just to the process of duplication, but to the process of recombination which, except for uniparental reproduction, is characteristic for all higher organisms.

[Some discussion developed among Mayr, Wright, and Weiss on whether recombination was a new principle or an extension of the self-duplicating principle. In any event, if one wishes to reduce the consideration of the uniqueness of biology to the distinctions between living systems and the inorganic, physical state, recombination is not pertinent since it serves only to distinguish one living system from another. We are here concerned with the minimum number of postulates that must be added, operationally, starting with the postulate that a living system is a molecular system.]

WRIGHT: Now, the viewpoint I've expressed is perhaps too simple. If you had a Watson-Crick polynucleotide formed, somehow or other, in a Haldane soup sufficiently rich that it could duplicate its pattern, and, because of the Watson-Crick mechanism, it duplicated itself just as easily if something does go wrong, then I'd say that you had life originating. Others might want to go a little bit farther and wait until you've got conditions of a three-body mutual interdependence.

WEISS: I'm one.

WRIGHT: But I would say that you had, really, the most essential thing, once you'd got a molecule with this property of duplicating its own pattern.

WEISS: Then demonstrate to me, in acceptable terms, that there is such a thing as self-duplication, that it doesn't already require an organized system, so to speak, outside, which helps in the duplication.

WRIGHT: You have physical situations surrounding it that form regularly in organic material under the particular conditions that . . . .

WEISS: Who sets those conditions? Who guarantees that those conditions would be satisfied?

WRIGHT: There isn't any guarantee. But some time in the history of the world, you did perhaps have conditions that produced these—what do you call them?—coacervate droplets.

WEISS: That's all right. So long as you said "perhaps," that's all right.

WRIGHT: But the essential thing is that, whatever the conditions, once you get the kind of a system that the Watson-Crick model provides, you've got the basis for individuality and history.

GERARD: But the more complicated the system that's reproducing itself, the simpler the soup.

WRIGHT: In a sense there is no limit to the complexity you can arrive at and you've got a fighting chance of getting these three-body systems, in which you have mutual interdependence, which couldn't possibly arise in any instant spontaneously. You've got to

approach it, it seems to me, in this way, whether or not you consider that life originated when you got this property of self-duplication in the sense that I implied.

MAYR: Would such a system indicate that life, so to speak, originated by a more or less fixed step as a single phenomenon? I mean, the theory includes implicitly that postulate, doesn't it?

WRIGHT: I don't think we know enough about that—that is, you've got to know just what the conditions were in this pre-living soup. It could only exist before there were any organisms; the organisms immediately proceeded to use it up.

HOTCHKISS: I would like to submit that replication isn't quite as sharply separated from physical phenomena as it may seem. You may think the analogy of crystallization is pretty well ruled out, but, as Weiss points out, replication never occurs in a vacuum and the soup must be rich. By the same token, crystallization requires a rich soup. When, say, leucine is present in a sufficiently rich soup, you have a supersaturated solution, and it requires one more thing—a kind of nucleus—to start the crystallization going. This is a process which involves the transformation of matter. Leucine in solution is not really very much like the leucine in highly ionized and hydrated form in a saturated solution.

WRIGHT: We aren't trying to set up an absolute distinction between nonliving and living, are we? What are the conditions for transition between the two? If it was an absolute break, of course, it would require a miracle for life to arise.

GERARD: Let us spend ten minutes or so on an issue which we have encountered tangentially over and over again. To what extent is biology to be regarded as relatively unique because it is concerned of necessity, and not by historical accident, with a great many individual cases, in contradistinction to the over-all category? It seems relevant that physics, not concerned with species of matter, is more unified than chemistry, which has to pay attention to a hundred odd specific constellations of the subatomic units. Thus chemistry is, in this sense, a more particular science than physics. Biology, of course, deals with enormously more complicated and more numerous particular systems. Is this an inherent, necessary aspect of biology, and are we kidding ourselves if we think we can get away from it?

GODDARD: Well, Gerard, I really think that methodologically, operationally, this is a very important aspect of biology. True, we do have populations that may be worked with, and they may be unique—a bacteriologist may deal with and study the properties of the culture as a whole. But, by and large, most biologists are operating with individuals and, what's more, these individuals are not identical. Usually they will be so uniform that we define them by certain terms, but we must recognize that each is separate. We also study individual processes or responses of the single organism. In sensory physiology, you study the response of an individual in relation to some change that you made in the environment. I think this is an important aspect of our science operationally; whether or not it's going to make a great deal of difference in our conceptual framework might be debated.

WEISS: I think it is fundamental to stress here why we are interested in these individualities. The physicist is constantly faced with similar phenomena, because he finds specific expressions of all the physical phenomena just as we do. He isn't, however, interested in those, because they are not recurrent, because they have no predictive value. Biologists are interested in species, individuals, chains of biochemical reactions, because there is a certain constancy or recurrence, which I refer to as organization. There is nothing queer about the observer, the biologist, that makes him interested in those things, but something inherent in the subject matter. . . .

GERARD: What of the meteorologist and storms?

WEISS: That comes very close to biological objects; the morphology of the thundercloud is nearer to biology than are the phenomena in which the physicist usually takes an interest.

MILLER: When you think about physicists, you somehow think about nuclear physicists and theoretical physicists. You

don't think about engineers, meterologists, crystallographers, geologists, and astronomers. There are several physical sciences that are interested in the specific.

WEISS: Miller, the machine is an object of organic nature because it is the product of the human mind. So, the structure of the machine does not appear in nature. Therefore, I do not include the engineers among the physicists. You see?

SCHMITT: Remember, Weiss, when you and President Conant and Philipp Frank had that symposium at M.I.T.? Professor Frank pointed out that people think of physics as an exact science, but this is true only in a statistical sense and that if you asked a physicist to predict when a particular dropped object, like a bit of paper, will fall to the floor, he would find it completely impossible. Yet, this is the kind of thing that biologists are faced with in working with organisms. No physicist would attempt anything so complex.

REYNOLDS: Isn't there another dimension coming into this, and that's the time factor? —because processes are essential even to the maintenance of living organisms. Therefore, in all living systems, you've got a different situation now than you had a few minutes ago, and will have still another system later on. This is true in a few instances in inanimate matter, but it's not universally true. Aren't these analogies with meteorology, waves, and a few things like that, in which there is this kind of similarity?

GERARD: I shall bring this discussion to a close with a little summary and some suggestions for the future. We have discussed one of the clearly primary issues, that of organization, in terms of information of second-order equations, and of the parameters of the secondary terms. We have talked a little about individuality, though not as much as perhaps we might, and we have talked some about the general and particular. It seems to me that we are smack up against the issue of an hierarchical organization of the world.

As a taxonomic job, we are trying to order and sub-order the universe of discourse. The decimal system, a major invention of man, does exactly this. As soon as a decimal point is introduced as a reference locus, the position of a numeral puts it in a class and all those to the right of it are subclasses. 0.3 has ten subclasses, of which 0.37 is one. This is used explicitly in Dewey's cataloguing system. The more the subdivision, the more the precision and, inevitably, the greater detail and particularization required. Now, work this the other way. The physicist concerned with the elementary particles of matter is dealing with a very high degree of generalization; only the mathematician, who doesn't have any particles, is more general. The next decimal, in my language the next higher level of organization, requires such particulars as needed to deal with the new major terms, and so on. Of necessity, chemistry is more particular than physics since a level of organization, a decimal, has been added; biology more than chemistry; and social science more than biology.

It has been brought out several times that there are other kinds of physical scientists than the physicist or the chemist. They, too, are concerned with more organized systems, at higher levels, but along a line different than the biologist. We should discuss whether the hierarchy coordinate in this table of levels is linear or whether, to include all of nature, it must branch above the molecule—in one direction to organelle or cell, the living systems; in another direction, to geographical, geological, astronomical, architectural, engineering, meteorological systems. Indeed, a similar branching seems to occur in the living domain above the individual—one direction giving species and higher taxonomic groups; the other, family, community, and higher social systems. Perhaps these two major points of dichotomy set off biology from the physical and social sciences.

[The Chairman assigned the following tasks as "homework."]

Will each of you list about five principles, concepts, properties, concerning living systems which you feel are relatively major in biology and which, at the level of maximum possible generalization—the numeral furthest to the left—subsume the more particular items? I suggest that, either before or after

you have made such a list, you refer back to the portion of my document which deals with concepts in biology, oriented in terms of levels and of the structural, functional or historical aspects of each level. Use it as a whipping post if you want, but at least this will give us some basis of interacting when we start.

SCHMITT: You differentiate biology from social science?

GERARD: Yes, just as I would differentiate it from the physical sciences. I am asking whether the essential difference is: (1) from the molecular level organization increases in two directions, one of which leads to biology and the other to geology, astronomy, meteorology, etc.; and (2) from the level of the individual is it a useful dichotomy to think of species, biota, ecosystems on the one hand and of social groups, especially human systems, on the other? Do they have enough specific characteristics to justify a real division even though as all biological systems fall within the framework of physical-chemical systems, so all social systems fall within the biological?

I spent a year at the Center for Advanced Study in the Behavorial Sciences with 34 social scientists. I worked hard to give them the realization that social science must ultimately build on biology as biology builds on physics and chemistry, but I fear most of them do not yet see this.

[The following morning, after several false starts, the group decided to go around the table, in order, presenting their statements of principles. Dr. Hotchkiss led off.]

HOTCHKISS: (1) The living biological units present non-random accumulations of matter.

(2) Living matter in appropriate environment tends to return to an average state whenever displaced slightly from that state. Maintenance of its average state depends more or less on continuous chemical transformation of components of the environment.

(3) Living matter in appropriate environments can increase in amount by duplication either in whole or in part.

(4) I was still working on a statement which says the order that you find is some resultant of the order that pre-existed in previous states of the system.

CASPARI: Well, I would start out with, "Life is found in matter delimited against an environment in the form of individuals," and second, "Individuals stand with each other in a reproductive relationship." That means individuals are able to give rise to other individuals. The third concept which I think ought to be in here is "variation." No individual is ever identical with any other individual. The individuals can be arranged according to their greater or smaller similarity in groups, in natural groups, which we call species and higher taxonomic orders. I already have mentioned the fact that individuals are connected with each other by the process of reproduction. In this reproduction changes are going on which lead to the phenomenon of evolution.

GODDARD: I'll start out with a very simple one: "Organisms arise from pre-existing organisms," and as subheads under the same general concept, "Cells arise from pre-existing cells,"—"Certain specific parts of cells arise from pre-existing parts." Under this I listed chromosomes, plastids—and mitochondria with a question mark.

WEISS: Centrosomes certainly can arise *de novo*.

GODDARD: Centrosomes arise *de novo*? Well, I don't know that I want to argue this, but I'm not sure about the mitochondria at all and then I went on next to biparental inheritance and said, "In sexual reproduction genetic material derives from two parents." We have subheadings under this: (1) genetic recombination occurs; (2) we have genetic segregation; and (3) we have genetic modification occurring randomly in any large population, without any attempt to give a frequency. By genetic modification, I am including mutation, chromosome changes, and so on. My next statement is that "the environment operates selectively in a reproductive population."

BRINK: Mine are all oriented around the molecular level. I tried to define a cell as "an open molecular system in physical con-

tact with its environment," and then, to complete the definition, list what I mean by physical contact, such as thermal exchange, molecular exchange, radiation. The total system is subject to the laws of physics. Each system exhibits properties of self-maintenance and, as a subheading under that, steady state properties (including reproduction as a mechanism of long-term maintenance). And the system is essentially a dynamic system of molecular transformations, emphasizing here that we exclude equilibrium as a major character. This is the convergence of the three areas, physics, chemistry, and biology.

HOTCHKISS: Would you say that these things are specific to biology?

BRINK: I think, that, at this level of analysis, this is not a sound position. Physical theory permits occurrence of highly ordered molecular systems provided there is a continuous net increase in entropy of system plus environment.

SIMPSON: I have, as was suggested, stated complex principles in some cases rather than concepts. First, "living organisms are physical-chemical systems that differ from others only in structure and complexity;" second, "organisms reproduce with internal replication of large, information-bearing molecules, the replication occasionally being not precise;" three, "the nature and diversity of organisms are the result of the historical process of descent with modification." In other words, they evolve. Fourth, "directional evolution is mainly, perhaps entirely, adaptive." Fifth, "genetic adaptation results from differential reproduction in populations, and the differential is correlated with variation due to mutation and recombination."

SPUHLER: I went in the opposite direction by giving labels to concepts. I deliberately attempted to choose these from the higher levels. They are some of the concepts which we tend to use as unexamined logical samples: "Individual," "population," "character" in the sense of a biological attribute or genetic trait, "symbol," and because it differs seemingly from other types of organization, "social organization."

SCHMITT: Living organisms: (1) Are capable of impressing upon their substrate environment their own specific ordering, morphological and chemical; (2) are able to produce regular, periodic and also characteristically aperiodic structure from amorphous or poorly ordered components; (3) have the ability to provide enzyme complements capable of dealing with the environment adaptively—demonstration of the historical aspect; (4) can couple chemical free energy with macromolecular structure, to do mechanical, osmotic, electrical and chemical work.

MILLER: My statements may not all be proved, but they are interesting; and some of them, like those of previous speakers, may also be true in some non-living systems: (1) The notion of levels that Gerard has emphasized, the formal identities among levels; (2) systems which survive are those which are capable of optimizing power output and sub-optimizing efficiency on occasion; (3) there is storage of information over time—that is learning—and this occurs at an optimal rate, somewhere between zero and one, where zero represents no learning, and one represents learning on the first trial, dependent upon the average probability of input from the environment; (4) the rate of return to equilibrium of any variable maintained in a steady state is a function stronger than linear of the amount of displacement from the equilibrium point; (5) equifinality is a principle which operates in the living systems which are open and feed on negentropy, in the usual definition of those terms.

REYNOLDS: I feel I ought to apologize for my first one—it's got this purposefulness in it. "Living matter is that which exhibits the property of tending to preserve its existence dynamically. The processes favoring this tendency have survived among the possible processes of living matter. Included are homeostatic processes—including such things as avoidance and social structure, reproductive processes, including speciation, which produces a stability, and genetic recombination and modification producing the variations. These, in turn, produce evolution, which tends to insure the initial things, namely preservation of as much living material in as wide an environmental range as possible."

MAYR: That's what you call a single package deal.

WRIGHT: Mine are rather lists of catch words, without trying to go into their meaning. I started with (1) the concept of an *organism* as an entity that persists in spite of continual change of material and continual changing environment, by virtue of correlative interdependence of its parts. (2) Such organisms in general *develop* from something very much simpler. Next, (3) the *hierarchic nature* of organization: that organisms are composed of organisms. This had its development in the last century in (4) the *cell theory*; that organisms are composed of entities which have the properties of organisms on their own account, cells which maintain themselves by virtue of metabolism and reproduce by mitosis. Hierarchic organization is now carried further in (5) the *gene theory*, which holds that the cells maintain their properties and their individuality through macromolecules that have the capacity for imposing their own pattern on suitable substrates. It goes still further to (6) the *continuity of the living with the nonliving:* the properties of genes, cells, organisms, including human beings, are ultimately just such properties as those manifested in calcium nitrate, calcium phosphate, etc. and are to be reduced to such terms as far as possible. That is, there are not two worlds. Of course, you can go into the question of whether this is essentially a denial of vitalism. Here is where I part company with Mainx. I would say that because of this continuity between the living and the nonliving, you've got to hold that amoebae, dogs, human beings, are dead as doornails, even if they don't know it, or you can go in the other direction and say that because we know that we are conscious, this implies the panpsychic principle, that hydrogen ions are as alive and conscious as human beings, that consciousness is not something suddenly imposed in evolution. From the standpoint of philosophy, the continuity of living with the nonliving to my mind implies this latter direction of thought rather than the direction along which Jacques Loeb was the leader at the time I was breaking into biology. I make a distinction there between a mere aggregation and something that is an organism in some sense (molecule, atom, elementary particle). From the standpoint of scientific method, however, I go along with Loeb.

In the other direction from the organism, the principle of (7) the *species concept* is that individuals, through biparental reproduction, form systems that persist and that have a history, evolution. Species, in spite of continual change of the individuals of which they are composed, tend to persist over a very long period of time, some millions of years, with very little change. The species is, then, an assemblage of organisms in which there is sufficient continuity of interbreeding to give an entity with this self-perpetuating property. The highest level in the hierarchy has been called (8) the *web of life:* the entire array of organisms in any locality, and through the connection of localities ultimately in the whole world. There is here a system of parts each of which has a high degree of independence but also mutual dependence. The 3-body problem comes in there just as much as at lower levels, in that none of them can exist by themselves. Finally, (9) the principle of evolution of species and of systems of life must be listed as one of the greatest biological generalities.

MAYR: First, is "the ability, in living matter, of precise replica formation, permitting inheritance, including continued replication of errors, which we call mutation." Two—"the existence of various mechanisms, in particular recombination, biparental reproduction, sexuality, whichever you choose, which in connection with the selective effect of the environment, differential survival and fertility and stochastic processes, leads to a continuous change called organic evolution. The characteristics of organisms can be fully understood only in terms of this evolution." Three—"the fitting, 'adaptation,' of each kind of organism into a limited subdivision of the total environment, 'ecological niche,' an adaptation which severely limits the total number of possible genetic variability and has favored the evolution of isolated gene pools called species." Four—"the ability of living matter to build, from simple compounds, complex

structures on the molecular and organic levels and the utilization in this process of solar energy for all energy requirements directly by photosynthesis, or indirectly." Five—"the uniqueness of units on various levels of integration, particularly that of the individual, so that aggregates of such units can only be treated statistically, 'populations,' not typologically, a fact of tremendous importance for the understanding of evolutionary processes." Six—"the presence throughout the living world of stabilizing mechanisms from the stable gene through homeostatic organisms to the well-integrated ecological community."

WEISS: Well, I'll try to stay out of philosophy and be operational, strictly operational. First, "life is a complex of phenomena and is a process, therefore the unit of life can never be a particle or static body, but has to be a unitary elementary *process* which has certain properties." The nature of this process is such that if we look at cross-sections of the process at any one time—that's what we mean when we look at a microscopic picture of an organism—its parts are momentary expressions of this cross-section, but the total process has a greater degree of invariance than the individual component parts. If it were a purely random system, then the variances of the individual parts would summate as we go from the individual components to the collective. What actually happens is that as we proceed from the parts to the collective, the collective shows greater stability or invariance. If this is true, then it immediately leads to the postulate of a hierarchy of levels. But this constancy or invariance is true not only at one level, a molecule or a molecular assembly, but repeats itself at the level of the cell component, of the cell, of the tissue, of the organism, and so on. So this statement goes for all levels.

This immediately leads to the next consideration; if there is this relative invariance at the collective level, there must be an interrelation, obviously, of the individual constituents. The parts are non-identical, diverse, that is what we call heterogeneity of composition. But the thing that is constant is not the composition as such, but the order of that heterogeneity; in time, it's the orderly sequence; in space, it's the orderly arrangement. Pattern, therefore, is more invariant than mere heterogeneity. So, I think orderliness of the pattern has to be added. I may say at the same time that the factor which produced this orderliness may not be a positive pattern-creating factor, but it may be rather the restraining of disorderliness, restricting degrees of freedom of the system, leading from the random to the non-random system of typical order. If the individual parts are interrelated in their reactions, they then establish ecological systems that are mutually helpful to each other, or at least compatible. If this is true on the level of society, organisms in groups, cells in tissues, then the same thing can be said of the elementary biochemical reactions within the cell. Therefore, I coined the term "molecular ecology" which is distinct from the biochemistry of an homogenate. Molecular ecology means that individual reactions are interrelated in such a way that what one does is an existential requirement for its neighbors, and *vice versa*. If this is accepted, the question becomes, "What established it?" and since one molecular group or species and its activities is the essential prerequisite for the others, you are in a vicious circle, which at present you cannot break into. Hence we cannot synthesize any of these systems from scratch.

This next leads to the introduction of statistics and an assertion of the non-identity of any living systems. The idea of identity of cells—any two cells, any two individuals, and so on—is a fiction. The problem of individuality is merely an expression of the fact that you do have this greater constancy at the higher level of the organized individual system than you have of its constituent parts. That is, identical twins are much more similar than are any microscopic sections from corresponding sites you can lay through either of them. This finally leads up to another concept which I am just hinting at. The concept of specificity as applied to a singled-out component is an absolutely meaningless term. Specificity is a term which points to relations. You can only have specific relations between two activities or two processes. Thus specificity is only

another expression of the matching, the fitting, the adaptation of two interrelated processes relative to each other—the "three-body problem" which we discussed yesterday.

Finally, the fact that these molecular phenomena interact on a large scale leads immediately to the problem of fields and field action. The concept of fields means that you cannot reconstruct the total behavior of a heterogenous group entirely from what you know about any of the components acting independently in isolation.

If you have a complex heterogenous group, and you know the properties of the individual components, you cannot derive from those components alone the resultant activity of the collective by sheer summation. This is what is implied in the concept of fields. It also implies that you do not merely get greater complexities as you go up, that biological systems are not only more complex as differentiation increases, but that with the increase in the number of components, there is a decrease in the number of parameters you need to describe the order of the components. You have constancy of order despite the increase in the number of components. The differential between surface and interior, between polarity and symmetry—all these things enter in. I've tried to point to a purely operational level, and I think this can be done, but biology is still far from doing it at the present time.

GERARD: My list certainly has nothing new by this time; my first item was a hierarchical organization of entities, in interaction, which would subsume the cell theory, the idea of gene pools, individuality and such things. Then the notion of autocatalysis or reduplicating units, emphasizing that this occurs at all levels from the molecular up. Whether random sorting at the gene level is important enough to deserve explicit mention as a subhead, I don't know. As a third principle, stabilizing action, or homeostasis, which implies as a subhead, adaptive amplification or trigger action, the ability of the system to mobilize and release internal energy in amounts ordinarily greater than the environmental disturbance impinging upon it and so directed as to counteract the displacement or strain. Here "purpose" enters biology; not in a sense of a purposer, but in the sense that the significance of events is understood in terms of the future more easily than in terms of the past. Fourth, we find "increase of ordering," "capture of information," seen in the development of the individual, the evolution of the species, the progressive variation of individual types. I suppose, along with that, "adaptation" should be mentioned, although it might deserve a separate statement. Finally, perhaps better mentioned earlier, living things are dynamic, persisting, open systems, essentially in equilibrium, which exchange not only matter and energy and information with the environments but also, in a sense, rotate or turn over their component units or individuals—as cells in a multicellular organism or individuals in a self-perpetuating species or other group.

[With Dr. Gerard's summary, the more or less formal presentations of individual lists of principles concluded. Following that, there was a lengthy discussion of miscellaneous points raised by the formal presentations. This debate was initiated by the Chairman in these terms:]

GERARD: What enormously encouraged me as we went around was that the variations in our statements concentrated overwhelmingly in two areas: one, the particular precise phraseology with which a common idea was given one or another emphasis; and two, an inevitable difference, owing to our particular interests, as to which ones of the few general principles were subjected to greater subdivision. With very few exceptions—such as bringing in rates of learning, or the notion of symbol, or the question of subjectivity—we have all said almost the same things. Would anyone disagree with that; do you think that we are not saying as nearly the same things as I think we are?

MILLER: If that is true, we have said it in a fascinating variety of ways.

GODDARD: In some cases, Gerard, I think our definitions are not in operational terms, but perhaps could be modified. For example, at least two people made the statement that no two individuals are identical. As a matter of faith I accept this statement; as a matter

of operational definition, I would challenge it. Not that I'm asserting that two bacteria in a population are identical—I would say we have no way of showing their non-identity. You cannot, as in an operational definition, really insist that there are no two organisms within a population that are identical.

WRIGHT: Even a hydrogen atom has a great variety of separate states. When we take all the hydrogen atoms and all the carbon atoms, and so on, and consider them in their various excited states, the chance that any two organisms will be identical are utterly infinitesimal.

MAYR: Sometimes when biologists say "identical," they refer to the genetic identity of asexually reproducing organisms. In other cases, as now, they really mean absolute identity in the strictest sense.

GODDARD: I recognize perfectly well you can have a genetic clone. The statement that two organisms aren't identical is a statement of faith unless one spells it out in operational terms.

Now, another case I would raise is the ecological relationship; I think it came out of something that Dr. Weiss and some others said. There are certainly very real reactions between organisms, but then the word "mutual" comes out, which is, I think, non-operational and could be dispensed with. There could be some other way of expressing the same concept in an operational sense.

MILLER: If we are going to discuss this point we need to consider the notion of the principle of identity, which is operationally definable in terms of locus in space-time. No coccus, for example, is located in space-time exactly in the same place as any other coccus, even though they may be indiscriminable in every other way. It's quite clear there will be different gravitational, osmotic influences, and so on.

MAYR: Yes, but in this sense it is also true for atoms, for everything; it loses complete sense.

GODDARD: Gerard, I have a feeling that the ideas voiced around this table, if they could really be worked out would go a long way to cover the purposes for which we came here. I also think there is considerable diversity; I don't think we all said the same thing. It's true that natural selection came out more than once, and many of the ideas were reproduced in different words, but some people thought of things other people had completely forgotten.

I think a refinement of some of the definitions, including my own, could lead to a good deal more precision. To give you one illustration, "natural selection" as it has been used in many biology texts is not defined in operational terms. As Sewall Wright has developed it, it certainly is defined entirely in operational terms. This seems to be what we want to do.

WRIGHT: The trouble there is if you begin to spell these things out completely in operational terms you've got to write a book. All you can do is list catch phrases, as I tried to do, and hope people will supply the operational terms.

MAYR: Of course, everyone has a different reaction to terminologies. I, for instance—and I know I disagree with several at the table here—have no use for the term "dynamic" whatsoever. Whenever the word "dynamic" is used I have a feeling it covers up some vast ignorance and a person should rather say what he is talking about.

WEISS: Well, it means in physics that there is an operative, interactive relation between the components of a system. The term "dynamic" has been misused by extending it way beyond this physical meaning.

GERARD: Physiologists use it a lot and some of them know what they are talking about.

WEISS: Let me answer Goddard on the point of identity first. I think it is perfectly clear that Platonean identity of the "ideal type," as absolute congruity, doesn't exist. What we consider a cell, anything of the kind, is merely a statistical formulation of a group of experiences. However much we measure and probe it, we will never find two cells where exactly the same, or corresponding, things go on in exactly the same coordinates of space. You couldn't even expect it. I think we should put it in probability terms. We should say that the chance of ever finding two cells that are exactly identical down

to the last component is infinitesimally small.

The second point is on mutualism between two systems. This has been expressed many times. It is artifact to dissect an organism or a cell or anything away from its relations to the environment. It would be equally an artifact to say that living system A affects neighbor system B, but that there is no reciprocal action. System B, *de ipso facto*, becomes part of the environment of system A. Therefore the systems are interrelated in some mutual way which has to be described, that's all.

GODDARD: All I want to say is one could get rid of the word "mutualism" and describe in some other terms the interaction between organisms.

WEISS: But "inter" means the same thing as mutual. The term "inter" means two directions, a two-way street.

GERARD: I have a comment or two here, then we must decide on how to proceed. The answers that Weiss and Miller and Wright gave to Goddard, throw out the baby with the bath. If, by individuality, you mean that two things are not identical because they are individuals, then you haven't said anything of the remotest interest to biology as such. If individuals differ because their molecules are in different states and occupy different time-space coordinates, this is a truism for everything in the universe. You mean something more than that, however, and you must defend individuality at this other level. As far as mutualism is concerned, you are asking for some kind of operational statement; that is, if you observe entity A and entity B separately and together, then, on the basis of any accepted criterion such as rate of growth, number of individuals, and so on, there will be a difference in the two situations.

GODDARD: Well, I would prefer to have it defined in these terms. This is partly perhaps a psychological reaction to the ecologists' extremely crude use of the word mutualism, particularly when they have gotten into such things as "mutualistic symbiosis."

MAYR: And the whole biome concept and things like that.

BRINK: The "individual" idea occurs in physics, too. All you are saying is that you have to set up some rules of averaging for discussing those things that are measured. You don't look at each individual, you have to set up some rules of averaging for the properties that you do work with.

WEISS: But, I have an answer, Gerard, on this. It isn't to be taken quite as lightly as you make it, because while it's quite obvious that things are not identical in the sense we have spelled it out, biologists, by and large, behave as if they expected them to be identical. They use radiation or drugs on a group of cells and are surprised that the same dosage doesn't hit all of them at the same time, that they don't all spring into action simultaneously and identically. Now, they shouldn't have expected it because of the variability of the system, you see. The biologists still act and operate in these terms.

GERARD: No, I didn't try to throw it out, I wanted to put it at a level that may be more meaningful. Individuality may be extremely important, but not because the molecules are in different states.

WEISS: Individuality refers to the uniqueness of the individual system; not to its identy with another.

GERARD: Individuality is the absence of identity. Just one more point, and I put it positively: To what extent is our acute awareness of non-identity and individuality, at the biological level, the result of our closeness, as observers, to the subject matter?

How would you like to proceed? Just continue this free-for-all for a while and see what happens, or push to see if we do come out with a few basic concepts of which the others are variations?

SIMPSON: We are, of course, a spectrum here. I find, for example, that I am closest to Mayr and next closest perhaps to Wright. I find that I am farthest from Schmitt and I honestly didn't understand what he was talking about. If some others also feel they are too far removed in the spectrum, wouldn't it be a good next step to pursue a little farther these things the other person has said that we don't understand?

GERARD: Sounds reasonable, does anybody object to that?

BRINK: I was impressed as I listened that there was a common content in discussing open systems; it was differently expressed but almost everybody had it in there some way. This would suggest to me that we might be able to get a mutually agreeable expression of its full significance if we made an effort; really round it out.

MAYR: I think we had essentially two levels; we had this molecular level that you particularly talked about, Schmitt, and we had the level of those who think in terms of individuals, populations, species, etc. which is the level that Simpson and I are perhaps most interested in. There you have two very definite interest groups and approaches, although there are some that connect the two.

BRINK: Something about the system language holds these two aspects together, I think.

GERARD: We are wandering from Simpson's explicit request. He is entitled to clarification.

SIMPSON: You went through this too rapidly for my own benefit. I'd like to have you take up those points and expand a little bit on each one of them.

SCHMITT: I am not sure that anything that has been said around the table here today is what might be called a manifesto, and I am sympathetic with Goddard in his statement that a lot of what we've been saying here are articles of faith more than precise scientific statements of facts.

Number 1 was "capable of impressing upon its substrate environment, its own specific ordering, morphological and chemical." I don't know a nonliving system that is capable of taking its environment apart and putting it together according to its own order, except in a very superficial sense. In other words, we have in the macromolecular complexes of cells and tissues and their components, a type of ordering which is unique to those cells and tissues. And this is produced by the living organism upon substrates which themselves have no ordering or, if they have, this ordering has been reduced first, and then the reordering impressed upon it. I think that may be a fundamental thing.

WRIGHT: This is the same thing as a gene duplication, and impressing of gene specificity on other macromolecules.

MAYR: "Simple compounds into complex structures on the molecular or organic level," is a very different way of looking at it, but essentially it portrays the idea. Now I think if that could be put in such a way that it would be acceptable to everyone, we would have a major principle.

GERARD: Here is a second item on which practically everyone has said something: there is a going open dynamic system, with the ability to build structure and substance in its image, to reproduce itself.

WEISS: Doesn't the old term "assimilation" take care of that?

SCHMITT: No.

WEISS: Assimilation means making similar. This is the implication of the term "assimilation." The macromolecule makes, out of these scraps, something similar to itself.

SCHMITT: Inherent in this is not only the reduction of the original ordered parts to more disordered parts and the eventual production of a new type of order, but also the interacting mechanism, which I think you referred to yesterday, wherein energy is provided at specific loci in order to produce the new ordering. It is not sufficient for a helix of DNA, RNA, or protein to have some molecule come to lie across it, as some information theorists seem to think, and bingo, you've synthesized a new molecule! It takes a whole series of intermediary steps of biosynthesis before that can be accomplished, and here I think you have the essence of some of the "living" aspects of organisms, namely this beautiful organization, in time and space, of the energy-giving reactions with what might be called the ordering reactions. I will coin the phrase, "ordered coupling of energy."

GERARD: This is much closer to something unique in bio'ogy than any of the statements that have been made. Your earlier formulation would not, at a trivial level, exclude crystals nor, at a somewhat more

complex level, would it exclude Liesegang rings.

SCHMITT: The item about aperiodic structure, the ability to produce regularly and characteristic aperiodic structure from periodic or amorphorous components, is a part of the same general principle.

MILLER: You are really voicing the notion of negative entropy?

SCHMITT: Yes. It is capable of producing a structure of a higher order of complexity.

MILLER: In other words less probable, much less probable, and yet regularly reproducible.

WEISS: Well, this is exactly the same thing as what I referred to as ordered heterogeneity.

HOTCHKISS: It seems to me the only mistake is in telescoping them all. For my taste, ordered heterogeneity is already two thoughts, and ordered coupling of heterogeneity is three. The statement, repetitive production of ordered heterogeneity, that's four. It is comprehensive, but not clarifying, to try to get all four into one sentence—we want to separate them, do we not?

WEISS: No, because it is the basic characteristic of life that we have to have all of those together. Any one of them alone wouldn't do. You see, that's why you have to deal with them jointly.

HOTCHKISS: To establish the elemental principles and get them out and think about them, we must look at them one at a time.

WEISS: I thought we were establishing the minimum necessary requirements.

HOTCHKISS: Then the most comprehensive statement is to say living matter lives; if you want to expand it a little you say as I did something like "repetitive production of ordered heterogeneity." But to analyze it we have to say "One thing living matter does is create heterogeneity; the second thing it does is establish order in this heterogeneity; the third thing is that it can produce this from an unordered environment," and so on.

SCHMITT: One of my points was the ability to couple energy with macromolecular structures and produce mechanical, osmotic, electrical, and chemical work. This is part of the more general concept of ordered coupling of energy to produce characteristic, aperiodic structure.

MILLER: That last one in comparison with the ordered coupling of energy is a corollary or sub-theory. We should perhaps distinguish between primary theorems and corollaries.

MAYR: But who is to decide? Everything that Schmitt is saying is part of one of my six points! On the other hand, Schmitt might very well feel, and I wouldn't blame him a bit, like saying, "Well, all the other 5½ points that you have, as far as I am concerned, are just one point."

SCHMITT: I would have said that.

MAYR: We come right here to one of the cruxes of the whole thing and this is a good point you bring out. Just how are we going to decide which are the theorems and which are the sub-theorems? We will probably have to let time decide that. In other words let us accept Schmitt's word that these are the theorems on the molecular level and the word of Simpson and myself that these are the theorems on the upper level, and thus get something which is properly balanced, because it is a combination of the two extreme viewpoints.

SCHMITT: I'm trying to put myself back in the middle of the last century when people were arguing this way about thermodynamics and when there emerged some very, very clear-cut laws of energetics.

SPUHLER: I've been hoping that we would soon reach the level of the cell at least and get above the level of the molecule. Perhaps we can get to populations later.

WEISS: Well, when I say molecular ecology of the cells, that's not on the molecular level, this *is* a cell, that's the way I have described the cell.

SPUHLER: When you go to a higher level, you can treat all of the units which were defined on the next lower level as identical.

WRIGHT: It seems to be a question whether you want to get at the fundamental things that distinguish living from nonliving. You try to cover the whole field of biology, from the molecular level clear up to the web of life, the whole system of life as an organism, symplasm; you just can't do it in five statements and spell them out.

What *are* we trying to do? To get at the fundamental differences between living and non-living and leave all the higher levels of life out, to leave out the species concept, evolution concept, and so on? Simply deal with the dividing line between living and non-living at the molecular level? You have to try to cover the field.

GODDARD: Wright, I would think that the problems of species, of evolution and of populations are very basic to some of the distinctions between living and non-living. I think that you can treat living and non-living at the cell level versus the random molecules of the environment. But, after all, we could work out, if we had a mind to do it, all of natural selection on a single celled organism. A biology that forgets species, natural selection, genetic variability, interactions of populations, is a sterile biology.

WEISS: Essentially, the emerging laws and rules of group behavior as we go up from the molecular interest sphere, to living things, molecular groups, cell groups, individual groups, species, and the isomorphism between these different levels puts them very much in one class as contrasted to what we find in dissolute, pure, molecular action. Therefore what we've been doing here, I think, is quite legitimate, because whatever we do at one level, is in a way symbolic or at least illustrative of what happens at the others.

WRIGHT: You don't have this holomorphism all along the line, though. An individual is composed of many cells, with a certain degree of autonomy and also interaction, but they all come from one cell. The population traces to the entire population made up of many individuals.

WEISS: But the rules are still very much the same, despite the difference.

MILLER: There is an interesting assumption implied in what Mayr said a moment ago. I would suggest investigating the fascinating notion that part of your principles collapse into one of his and vice versa. I throw this out as a possibility, that the smaller the systems, the more important are the forces interacting between the units—that the larger you get, up toward group phenomena, the less strong are the forces interacting between the units as compared to the forces within the units. In other words, the ratio of strength of interunit-forces to intraunit forces differs at the different levels.

WEISS: I totally disagree with that, Miller, as a concept of the cell. I think what you call interunit forces are not a blind play of random-scrambled units, but even on the cellular level their degree of freedom is restrained by the group activities of the whole population, if you want, the molecular population of the cell. These restraining influences of the group on the individual are present in the individual cell and cell part, just as they are in the higher grouping. I think there is much closer isomorphism than you indicate.

HOTCHKISS: I think, bearing in mind Mayr's breakdown of our group, we could rather quickly homogenize or integrate the lists that three or four of us have made. So I suggest, to get ahead, we could study Brink's list and see if there is anything missing, anything we molecularists want to put in, then go right on and see how far they are fundamental.

I could illustrate what I mean perhaps. This idea of individuality of the organisms has been pretty well debated and maybe doesn't have to come up again. It was an attempt to say that organisms are sufficiently complex that you can't just superimpose one upon another. This, to me, depends upon two propositions: the nonrandomness of the order; and the fact that processes are going on. These are two fundamentals that we have put in our frame. There is an ordered heterogeneity, and there is a lot of conversion going on, and if you have this you *can't* have two individuals that are exactly alike. I feel that these things at the molecular level, ultimately determine uniqueness.

SCHMITT: I agree. This is self-evident, and it seems to me what we are trying to get out is something that we as biologists would feel is strictly unique. When you think of the macromolecule, or any particle, you have to consider the field around it. One of the things that is specific about protoplasm is the specific field around macromolecules;

that is, in fact, what does the ordering at that level.

We have the same thing at any higher level; we have ordering. One might say specificity equals ordering, individuality, and so on. What is it that communicates? The field around a specific macromolecule "communicates" with another macromolecule, which is capable of reacting in a specific manner with this field. You and I communicate, we interact, with each other in very specific ways. This very process of communication which at the lower level will be explicable eventually on states of the atoms and their ordering, may be explicable at the upper level not merely in terms of our nervous system and sensory mechanisms, but perhaps also on properties of such organized systems at present undreamt of.

WEISS: I see a bridge between what you and Hotchkiss just said. Let me try to develop it. There is the other side to your story which hasn't been mentioned. You had the two individualities which are not congruous in their units, so to speak—one couldn't conceive of congruity. Yet to call them partly identical or similar means there is enough resemblance between them to give them a common name. If they were unique, something that occurs only once, you wouldn't give them that sort of a name, right?

Now you say, obviously, as you go down to lower orders of magnitude, there is a greater variability. You can't reverse this procedure. If you take two similar complex systems, let them operate for a time at random and then compare them again, you would find that they have become so dissimilar that each would be unique and you wouldn't even know that there is such a thing as an individual. Therefore, in order to get them to the point where they remain similar, there's got to be something to the system which holds it tied together as a bundle of oriented, rather than random, processes, which leads us to the question of systems and system theory.

Let's forget for the moment about cells and organisms, and think of a chain of processes consisting of n steps in sequential order, a metabolic chain if you want. In the sensory cell reaction, the parameters of electrical and chemical changes are ordered in a particular way; then communication consists of the fact (and is only possible as long as you have this principle of individuality) that you have two systems which are similar in their parameters, so that when one system is releasing something, another system can resonate so to speak with it. I think the problem of specificity, the problem of individuality of responses, and what you were just saying about communications in the living systems, are really expressions of the same principle, that we have similarity in a shape or form of organization, you see, which has a certain character of invariability, of constancy.

MILLER: What Weiss just said fits exactly into the classic Shannon formulation of a transmitter, a receiver, a channel, a message and its negative, noise. The message constitutes the transmission of form or information from the transmitter to the receiver.

WEISS: The receiver is preformed to respond.

WRIGHT: It seems to me that, even at the molecular level, the special significance of the gene as a macromolecule should be recognized. Pneumococcus carbohydrates, no doubt, are imposing patterns on one thing and another, but only temporarily, over a short period. Nucleic acids is the thing that regulates pattern over the long run, all through life apparently, if we are right in supposing nucleic acid is the site of specificity of the gene. Polysaccharide can't carry on any great period of time. Nucleic acid can apparently impose pattern on the whole cell and maintain that over a long period of time. It seems to me even at the molecular level the special position of that particular macromolecule ought to be recognized.

SCHMITT: To me, this is all very stimulating but where does it lead? Out of this can't there emerge some concepts which may be productive in some new direction?

GERARD: This is close to what was on my mind. The talk at the end was about communication and information, and I confess to a certain sense of frustration. Over and over again, something was said, quite ex-

plicitly by one—and later was repeated by someone else, in slightly different words, as if it were a brand new thought. Why are we failing to communicate with each other to that extent? After we made our separate statements of principles I said we were all stating pretty much the same thing, but each was expanding his own area. An hour later this statement was made again as if entirely new. There should not have been any great communication problem here!

[As he had earlier indicated, the Chairman appointed Hotchkiss, Mayr, and Miller to act as a subcommittee during the coffee break to see what synthesis could be made of the separate lists. Each member was asked to provide this subcommittee with a legible copy of his original list of principles.]

[During the coffee break, the subcommittee attempted to reconcile the several lists of principles into a single, relatively simple, trial framework. The result of their effort was presented to the full conference group on the blackboard as follows:

I. Hierarchization of biological entities
  A. Entities are populations: consequently statistical rather than typological
II. Order
  A. Heterogeneity in interaction arrangements
  B. In energy supply and enzymes
  D. Detected by ordered observer—specificity
III. Stability (homeostasis, equilibrium, steady state)
  A. Open system
  B. Equifinality
    1. Efficiency—power
    2. Rate of return to stability
  Adapting (See IV)*
IV. Adaptation*
  A. Adaptive amplification
  B. Information storage—learning
V. Replication of similar individuals
  A. Pre-existing individuals interact with random factors
VI. Evolution (genetic alteration of molecular structure under selective impact of environment)
  A. Recombination of individuals and species Adapted diversity (See IV)*

Dr. Hotchkiss acted as spokesman for the subcommittee.]

HOTCHKISS: It looks as though hierarchical levels was felt to be a basic thought starting at the molecular level, through cells, tissues, organs, individuals, populations, to society. Order is perhaps the first criterion by which you recognize a living thing—you only give it a name when you begin to see some system in it. Stability becomes one of the next facts, because this order is repeatedly observed, and it tends to be maintained even under slight deformation. Because it is ordered, and because this order is more highly arranged than the environment in which it is transformed, it can be thought of as an open system, something which has concentrated matter in various forms from out of a less ordered environment. Almost all of the things we included describing living matter are perhaps based on the thought of its holding a structure or not quite holding a structure, increasing or replicating that tructure in an environment which is simpler than itself, and therefore doing some organizing and doing it at the expense of the environment.

GODDARD: I'm not acquainted with the word "equifinality"—I'd like somebody to tell me what it means.

MILLER: Equifinality is a concept, emphasized by Bertalanffy, which deals in mechanistic terms with what appears to be purposive goal behavior. It is perhaps best illustrated by an example. If you have a closed chemical system of silver nitrate and hydrochloric acid, and you have at time *t* a given amount of both, you can predict the rate of precipitation and also the total amount of precipitated silver chloride that you will get. If, on the other hand, you have an open system with an infinite environment of either silver nitrate or hydrochloric acid, then the system will come to a steady state or rate of precipitation, no matter whether you have a teaspoon or a ton of either component at the beginning. This equilibrium will be determined by dissociation constants of the components in the system and, therefore, no matter where you begin you always come out at the same, "equi-final," end. It may appear as if this were the purpose of the activity, as if the system were working toward this goal.

MAYR: Now that this is all on the blackboard, it occurred to me that from II to VI

* Debate soon established that "adaptation" should be split and transferred as indicated.

might possibly be simplified into three headings: the creation of order; the maintenance of order; and the changing of order. This would give you, I think, just about everything in here.

GERARD: And if "hierarchy" is a different coordinate, this is exactly the table I offered you at the start!

MAYR: It's an indication either of your brilliance or ours!

GERARD: Not at all—it shows that there is some sense to this way of dividing things up.

WEISS: I have one amendment, though. When you speak of order you really have evolution in mind but I think the change of order is equally characteristic of development. So when you speak of stability you have to consider a relative stability with base line changing unidirectionally.

MAYR: This is taken care of by No. I, you see. Change of order on the level of the individual, or within the individual, is differentiation and development; on the basis of populations and so on, it's evolution.

WEISS: That's right.

WRIGHT: I don't understand what you mean by recombination of individuals and species. I suppose I think of recombination in the genetic sense. If you are going to talk about hierarchization you ought to talk about the levels of the hierarchy, whether it's cell, organism, or so on.

HOTCHKISS: My feeling really was that recombination didn't belong in the early part since hierarchization is essentially a framework for the other subjects. Recombination is a rough term for a process like adaptation and evolution. Now and then one genetic line intersects another and there is recombination or reassortment; it is a new principle, because the others are self-contained and here is one that develops a discontinuity.

MAYR: We stumbled over the word "discontinuity," which had been stuck in there, and somebody said, "Well, discontinuity and recombination is the same thing," and then "recombination" was stuck in there where it certainly doesn't belong.

WRIGHT: The second one [I-A], "Entities are populations: consequently statistical?" They contain populations, molecular ecology, and so on but they are certainly not populations in the statistical sense.

WEISS: You call them random populations if the randomness were implied as an operational concept. Actually, the fact is that you speak of "random populations," and I know that, genetically, you never use redundant or tautological terms. If populations, as such, were random you wouldn't say "random populations."

GODDARD: What do you mean by "adaptive amplifications"? I know what you mean by adaptation, presumably, but what do you mean by amplification of adaptation? Is it that one is built on another? You don't mean that it's propagation through the individuals? What is meant?

HOTCHKISS: A response that becomes larger than its origin in the sense that it is reiterated, whether by propagation or by neural response patterning, accumulation of metabolites, or so on.

WEISS: Do you mean by adaptation the *process* of adaptation or the *state* of adaptedness, of being fit? These are two different things. The word "adaptation" doesn't mean a thing to me unless you specify it. If it means "fitness," simply, then it's implied in the term "stability" because if they weren't fit they wouldn't be here, there would be nothing to study, there would be no observer. The process of adaptation, of course, is a different story.

MAYR: It was meant in the sense of evolutionary diversification; the original item was more or less lifted out of Simpson's list and we had a feeling that, somehow or other, adaptation would be right under evolution. Evolution is not merely change but reaching a state of adaptedness and utilization of different components of the environment, which is a very important concept.

HOTCHKISS: I was thinking of it there (at III or IV) as responsiveness to environment *within* genetic limitations, and under evolution (VI) as responsiveness to environment with alterations of genetic limitations, recombination being one of the processes of evolutionary adaptation.

SIMPSON: If this is used as I used it, then

neither subhead is appropriate. Neither subhead comes under the usage of adaptation in my list of principles. I was using adaptation in the sense of evolutionary diversification, adaptation to different environmental niches, and so on.

MAYR: Recent usage of the term adaptation by microbiologists for changes within an individual is, in many ways, very unfortunate, I feel, because it has given the word "adaptation" two entirely different meanings.

CASPARI: What we are finding then is that the word adaptation is used in two quite different senses by Hotchkiss and Mayr.

GERARD: "Adaptive amplification" was my phrase, and it was in my list under the heading "Stability and homeostasis."

HOTCHKISS: Yes, but we felt that there were exceptions to stability and when constancy was not maintained, one should attempt to organize the observations in which there are modifications in organisms. One of these is response of an individual to environment, another is response to more deeply determined—say genetic—things where the environment plays a part in selecting among individuals.

MAYR: Well, I feel strongly, Hotchkiss, that your way of looking at adaptation belongs strictly under stability.

WEISS: That's one factor; but it's not the whole story.

MILLER: Would anyone mind our combining stability and adaptation, and putting another kind of adaptation down under evolution?

GERARD: So "behavior" has to be split—this is something that I have come to in the last year. I had originally used adaptive amplification as equivalent to behavior. This is not adequate; it is useful to divide behavior into adaptive behavior and innovative, or creative behavior. Adaptive behavior is part of the process of homeostasis; creative behavior obviously leads to a new status which will then become stable.

WEISS: I would like to make this dichotomy clear. Under your point VI "evolution," adaptation would come about as a state of adaptedness without necessarily having anything to do with goal-seeking. It is simply that the outcome of change either fits or doesn't. If it doesn't, then the products of that change will be eliminated; if it does fit they will be preserved. The preservation of the adapted state does not necessarily presuppose a process of actual adaptive change, as we have it in reactions under II, that maintains only homeostasis.

GERARD: One could say exactly the same thing about creative behavior at each level. What occurs at the level of behavior of the individual over a small time period is paralleled for behavior of the species over a long time.

WEISS: Except that you can assume that under III it has been developed as a result of evolutionary development of adaptedness. So organisms which have the capacity of homeostasis, so to speak, have been preserved; so you could subsume it only under VI, then.

MAYR: Couldn't we now simplify this by eliminating Item No. IV and putting part of it under III and part of it under VI?

HOTCHKISS: Or we could do what was said earlier—production of order, maintenance of order, and change of order.

MAYR: Yes, I think that at the same time we can take out adaptation, which is part of the stability scheme, and then we have what I suggested.

WEISS: But, Hotchkiss, change of order needn't be adaptive; it may be disruptive.

MAYR: This is one of the subdivisions of change of order.

WEISS: It's the final result that counts.

MILLER: In the period of three-quarters of an hour we [the subcommittee] succeeded in getting everything on one blackboard, but I don't think any of us has subjective conviction about the arrangement and any alterations will be greatly appreciated.

HOTCHKISS: I wonder, too, about how we can incorporate the developmental part of stability—stability not in a single state or environment but in a series of them. The stability of a sea urchin includes its stability at various stages under various conditions. Don't we want this under stability?

MAYR: Well, we have stability at every

level from the molecular level up to that of the ecological community.

HOTCHKISS: Not only at the levels of different creatures but also at different times for the same creature.

WEISS: You have homeostasis at any one state for short time samples. If you take a short time sample you will have homeostasis, the system will try to maintain its state within that chronological standard. If you use a larger chronological standard you will not have homeostasis but you will have progressive unidirectional change of the base line. And homeostasis is a fluctuation around that base line. The system reacts, but the properties of the system change gradually. It does not necessarily return to exactly the same state, but you can't measure the differentials within short time intervals.

GODDARD: Weiss, I agree with this, and it seems to me that this base line change is either in development or evolution.

WEISS: Both, yes.

GODDARD: But, the one thing that is common is that in the change it never reaches equilibrium with the environment. We still maintain the dynamic equilibrium. . . .

WEISS: Except in death.

GODDARD: Yes, except in death—only the dynamic equilibrium is not a constant with time.

WEISS: That's right. There is a residual tension left which is not included.

GODDARD: In fact if there were not a change in homeostasis there would be no development; there would be no growth.

WEISS: The diapausing insect or something of that kind is closest to absolute stability but that is a very rare exception.

GODDARD: If you eliminate IV we must modify III enough to recognize that if we put limitations on homeostasis, the concept of learning is not thrown out. It is this concept that we call information storage.

MAYR: Everything else could be subsumed under "maintenance of order."

GODDARD: Then we should spell it out.

WEISS: The trouble seems to be that order, in this sense, is conceived as essentially space order and when you speak of homeostasis you refer to space order. Once you conceive order as a four-dimensional order in the time space, then your unidirectional change is part of the order. The system evolves in time, just as you have coordinates in space.

GODDARD: This is predetermined by its genetic make-up.

WEISS: Exactly, and if you include this as a four-dimensional order then stability is all right because you have to refer to stability with regard not to the space coordinates but to the point on the chronological line where the system happens to be at the time.

GODDARD: That is what you do whenever you use a developmental index.

WEISS: Exactly.

GODDARD: My only point is that if you want to discard IV we ought to amplify number III enough to include it.

MAYR: Both III and VI.

WEISS: Of course, one problem here is that there are really two principles incorporated in the table. One is the general properties of any and all living systems that *may* ever conceivably exist on earth; and then under point VI you are dealing with specific sectors that have actually *been* created on earth. Everything from I to V deals with the general rules of living systems and VI deals only with this very narrow finite number that have actually adaptively developed.

GODDARD: VI deals with two things. VI deals with the process as well as the result. It is the difference between Darwinian evolution and current evolution.

WEISS: But when you introduce the term "adaptive" you are dealing with a specific species and so on, because they are adaptive. Non-adaptive species won't last, you won't be able to study them—they could arise as possibilities, but wouldn't become species.

GODDARD: Even an ordered system breaks down and becomes disordered—this is one of the aspects of it when it dies. It maintains homeostasis, but homeostasis fails, so this must not be entirely unidirectional. It is generally unidirectional in growth and development but in many of these things we are dealing with a continuum which may

fluctuate in either direction. And this happens in evolution. You can have the evolution of forms that are both adapted and unadapted to the environment and their reproductive probabilities are different, so they are preserved or eliminated. The same thing happens with the behavior of an individual; bacterial cells die in a population. Thus homeostasis is not complete.

SIMPSON: The particular aside I was making, which of course I shouldn't have made, was that I don't believe that you can create even one individual organism that is not adaptive. I think that adaptation is completely general, as much as I think that order is. It is, in fact, inseparable from the idea of order. I think that evolution, as most of us have studied it, is not quite as specific as Weiss's remarks might suggest. I, at least, think of it as a general historical principle that order has changed, that everything else we are discussing here also has an aspect of change through time. In this sense it is a completely different kind of category from the others because it is something that *applies to* everything that has gone before it as well as having specific aspects which are *distinct from* everything that has gone before it.

WEISS: That's implied in what Goddard said. In point VI, actually, you imply both the general mechanism of permanent change—evolutionary change—and the specific expressions it has found.

SIMPSON: We not only have the general concept of order but we have specific examples of order.

GODDARD: That's one thing that bothers me with No. I. I still find that No. I is too broad, at least for me, to include all of the taxonomy of organisms under it as well as hierarchical orders in the structural components of the cell and combination of cells to tissues, organ systems, and so on. We all know this ourselves, because we are biologists and have had biological experience.

WRIGHT: It seems to me that unless you are going to lose yourself in abstractness, you've got to make your system two-dimensional and have hierarchy from gene to cell to a whole system of life (as one coordinate) and then these various problems that arise at each level at right angles to it. Thus you have the problem of homeostasis, persistence in the kind of entity, whether it is a cell, an organism, a species or a whole population, but taking quite different forms. You have the problem of change, of history, and it seems to me that its manifestations are so different it is desirable to separate the fluctuations—the persistent aspect, the homeostatic aspect—from the secular change aspect. So you have (1) the description of the climax phase (histology, anatomy, taxonomy, etc.); (2) homeostasis, the means or mechanism of persistence at each level, gene to whole population; (3) the description and (4) the interpretation of history of, say, an individual as the whole developmental process or evolutionary process for the species. The history of biota is evolution in the sense of Lotka in which each species is just an element in an evolutionary process of a system of life. Carnivores, herbivores, and so on are evolving as a coordinated integrated system. At least at the lower levels you have (5) reproduction, including duplication of genes, of cells, of individuals and the splitting of species in evolution. Really to get away from such abstractedness that nobody understands what you are talking about, unless you are there to explain it, you have pretty nearly got to make it two-dimensional.

GERARD: I won't quarrel with you. This points up the levels, so much spelled out. Here is structure, essentially; here is what I have called function, that is equilibrium—state of maintaining itself; and here, already slipped between them, is history, whether you talk of the short-range over the individual or the long-range over the group. This is the table that has developed. We all seem to agree with it. Now maybe we can go ahead with the second half of the program and get down to specifics.

[With the above remarks, the Chairman brought to at least a temporary halt the rather random commentary on the preliminary outline and directed attention to what might next be done with it.]

We can, then, as Goddard wanted, start looking at particular ideas, in the different

rubrics, "growth," "evolution," "equilibrium," or "homeostasis," and see whether certain meaningful commonalties exist. It has been useful, for example, to see that the words "adaptation" and "behavior" can be split into two, representing stability and change factors.

WEISS: I can see its great helpfulness for us, but where do we go from here once we have this? What are we supposed to do with it?

GERARD: We can spend some time ourselves doing a pioneer job, exemplifying what needs to be done in order to take this great mass of terms and ideas and put them into some rational system.

MILLER: To some degree I listed different sorts of things from a number of others. Consequently the notion of the efficiency and power and the notion of rate of return to stability are indented doubly in the outline, simply because these are pretty specific generalizations. The statement that systems that have survived from an evolutionary point of view can subordinate efficiency of, say, energy utilization, to temporary powerful output is quite specific. So, too, is the statement that rate of return to stability increases more exponentially than linearly the farther the variable is from the equilibrium point. These are just two of many possible generalizations that are either true or false about all levels—and I am not sure which. We would look for more specific statements about biological phenomena which fall in one place or another under these general rubrics.

WEISS: Who is to do the looking? I am just asking tactical questions now and would like to see how they are going to come out.

GERARD: If you will look at the agenda for a minute, gentlemen, you'll see just where we are. We have talked over objectives, we have talked over the role of conceptual analysis; we have, in effect, just now chosen a set of coordinates, and we are ready for the third item, under general analysis and subdivisions of the coordinates. What we have done, in effect, is to reverse the analysis. The group came up with six items; we have seen that these can be combined back to three. It is important not to leave them there, but to see how they divide up significantly into subheadings. Maybe we want to examine each one of these in more detail. For example, V and VI are both history, but the history of the individual and the history of the race are quite significantly different.

WRIGHT: V is reproduction, isn't it? Yes, I think it's useful to separate that from history.

WEISS: I think this is a very useful operation, but is it going to do the trick? Suppose we go into further particulars. Is that going to convince the pragmatic die-hards, who are not used to this type of operation, to continue?

MILLER: I was trying to say that if you get down two or three levels more specifically you will suddenly, I suspect, find yourself coming right into classical experimentation that has already been done, and is being done. In other words, the illustrations you think of are experiments which have an important status in the field of biology.

WEISS: Agreed, Miller. We, here, are agreed. We have already gone through this and we will go further. But is it going to be persuasive; is somebody else going to carry on from here? Or are they going to consider this some kind of a parlor game we are playing here?

GERARD: I don't understand your question? What is it you are trying to ask?

WEISS: Is this going to penetrate into the education system—can we consider this type of operation sufficiently significant to be taken seriously all across the country and carried on? I agree it is very useful to us, but we have been selected because we know that it would be useful to us.

GODDARD: Weiss, can I give a two-bit answer? It happens that I had my physiology as an animal physiologist, though I practice plant physiology. As a result the concept of homeostasis seems to me very meaningful in relation to plant cells and I use it in my teaching. But, except for my own papers, I do not think that you will find the concept of homeostasis occurring anywhere in botanical literature! If you will examine what really happens in a plant cell, even to the level of

regulation of glucose concentration in a barley leaf, there is as beautiful a homeostatic mechanism as in a mammalian bloodstream. Now it seems to me that if we could get my botanical colleagues to realize that some of the concepts developed by animal workers are equally applicable to their systems, it would be of great value.

SCHMITT: Now, wait a minute, Goddard, are you going to get your colleagues to realize that by looking at a three or x-dimensional scheme of some sort?

GODDARD: You're not going to accomplish that by publishing what's on the blackboard.

WEISS: That's my question, Goddard, a purely tactical question. We have only a day and a half, and have reached a nice midway point, and I want to raise the question what will come out of it.

SCHMITT: Would this be valuable, for example, to a physicist who is interested in this field and really wants a short-cut for a whole education in biology? Would it be a basis for coming up with something really fundamentally new in biology, by asking this group, as Simpson said, to impart wisdom or, you might say, originality? I think Weiss's question is perfectly legitimate.

MILLER: I think so, too. And Goddard gave the right answer to it, which is that such work will make evident priorities, both in emphasis in further research, in criticism of previous research, and in teaching. He pointed out, for example, one sort of thinking in botany which is not so explicit as zoology.

This would suggest that here is a part of botany which should have priority, conceptually. You might find studies of animals at a given level where one of these areas has not been prosecuted. Therefore, if somebody comes in with a proposal to do research in an area which is an obvious gap up to the present, it would be given priority. In organizing teaching, priorities might be made in this manner rather than in some of the traditional ways.

WEISS: I realize this, but how is it going to be made effective? Is Goddard going out and go to the botanists and give them speeches or lectures under this scheme?

BRINK: As a partial answer, if all of us in teaching or in writing research papers were to formulate our problems and present them from this point of view, it would influence more and more people.

WEISS: Exactly, Brink. If one finds, in trying to write one's research paper that he can't fit concrete facts within this sort of conceptual framework without stretching it a bit, well, then, he discovers there is something wrong and in this way is forced to reexamine its validity.

REYNOLDS: It seems to me that in filling in the particulars we are in a way going downhill. We have been going uphill all along. Are we at the top of the hill yet? Have we got as general as we can get? I think we ought to do that.

SCHMITT: To stay with Weiss's question for one second more, those who are trying to develop biophysics are faced with the problem of people whose discipline has been physics and who are coming into biophysics. They may be very bright, in many cases brilliant, but have no background at all in biology. Now the question is are you going to burden them with all kinds of details—comparative anatomy, to pick on that as an example—such as the undergraduate curriculum has unfortunately been burdened with, or are you going to start something like this, using copious illustrations, over a one-year period? And won't this be better than a whole four years of undergraduate details? I think this is a very definite, positive contribution.

HOTCHKISS: There is a vast amount of empirical information in chemistry; antimony triiodide, and cadmium sulphide, and all these things have their special properties. But this has long ago been ordered into a framework so that the average student comes out having at least that framework from which he can learn principles, and with which he can remember more details.

SCHMITT: Except that some descriptive freshman chemistries are just as poisonous as ever. Does freshman biology have to continue to be poisonous?

MAYR: I agree with Schmitt 100 per cent. One of the most useful things in any field that suffers from a surplus of detail—and there are very few scientific fields that

don't—would be to devise a conceptual framework under which this multitude of facts can be pushed so that the framework is always visible, so that the people know what it really all means. If we can do this, well, we have gained something.

SCHMITT: If we had some real flesh on the skeleton so that it isn't a purely formal outline, but well illustrated, I think it would be wonderful.

GERARD: All right, are we going uphill or downhill at the moment?

MILLER: Let's proceed to set up—I thought they were good all along and might as well admit it—boxes where you have hierarchization along one set of dimensions and items II and VI along the other set, and then think of specific illustrations.

GODDARD: I don't believe this is downhill.

REYNOLDS: I do.

GODDARD: Because it's ordering knowledge.

REYNOLDS: I am wondering if there are any further generalizations. Except for the concept of order, which we've used to be more or less synonymous with life, all the rest of these are things that tend to maintain order, which has also been given the word "survival." Several people here have used the term survival or survival value in discussing various components of this as they go along. I am not saying this is a new thought, but at least it seems to be a thread that goes through all this, and maybe there is something more general yet that we haven't gotten at.

WEISS: Well, there is still a great deal of clarification needed in regard to some of the terms that are being accepted here. Consider, for instance, "systems"—open systems, closed systems—which are coming up in the world so rapidly. Since we have spoken of the disregard, or ineffectiveness, of writings, I may be permitted to point out now that I started thinking out this system concept in 1922–23; it was published in 1925 in a very elaborate paper. You will find there many of the essentials of what is now being widely discussed as system theory for behavior, as well as for development. But the fact that it has remained practically hopelessly ineffective and unnoticed, except by a few psychologists in the 20's, is something which makes me wonder about the tactical value of this present type of operation. What can be done to make such efforts as our present one truly effective so that we really get it spread among our public and carry the message around?

CASPARI: I want to challenge your statement. I know very well that we as graduate students read the paper and discussed it thoroughly.

WEISS: That was in Germany. Let me return to the main point: When you speak of a "stationary state," or of stability, where a deformation or removal from that state automatically evokes a reaction of opposite sign among parts of the system which tends to conserve the state of the whole, this leaves the "state of the system," as such, undefined. The conserved state is conservative only for the time sample that has been chosen and it doesn't mean that one system cannot be converted or transformed into countless other systems which have equal degrees of stability. This is not what you find in a living system, which does not have a large number of equivalent system conditions. You have usually only one unique condition, which characterized the system at any one phase of its existence. And for this it takes additional parameters, up and above those we generally concede to systems. And those are the ones that you would refer to as "field" characteristics. Field parameters are something that must be added to the description of the state of a given system to make it complete. The field is a system, but it's more, it's a specific kind of system with fewer degrees of freedom than those permissible to an ordinary non-living system. So it takes something more than just a system to constitute an organism and that's where the concept of fields, if you want, comes in. Now I don't find this, for instance, anywhere differentiated here. How much additional polishing or pin-pointing does it take to get the main categories worked out?

SCHMITT: We have been discussing the formation, maintenance, and change of order—call it system, field, or whatever. We should also state that there are manifesta-

tions of order about which we have little knowledge. Memory, for example, must result from an interaction of some kind of ordered system. Where are the great challenges? I think that this is really expressing what all of us had in mind—it isn't enough just to classify and box. For the student a real challenge must be given—not in the sense that the great minds have been able to order the concepts of biology, as such, but have come up with something that is really new.

SPUHLER: It seems to me that we have made quite a bit of progress this morning, and I would suggest the next step is to consider the hierarchies or the levels as a specific problem.

[The Chairman summarized the foregoing discussion and outlined suggestions for subsequent efforts in the following terms.]

GERARD: May I suggest that while we may be able to go further uphill and get additional generalizations, it has taken us a whale of a lot of time and effort to get this far, and we are in danger of taking all of the rest of the time we have. I think we will do much better now to take essentially this framework and particularize these glittering statements which, in a day and a half (on top of all our experience) we have reconstituted. Go backwards and say, "All right, what does this mean in actuality? How does this work out in botany at the cell level, in zoology at the species level, and so on?" In this way we come back to the biological public, student and teacher, and get down to the concrete items and instances.

I suggest that we start this afternoon looking at the document you have. At the end of it, a number of pages are each divided vertically from I to IX in terms of levels. As Spuhler has just said, it may be worthwhile to examine this dimension a bit further and see which of these suggested levels are significant and meaningful. Across the top are three major categories, which can be reduced to structure, function (I don't care what words we use), and history. And on each page is a list of items under, say, structure, and organized by levels. There are three sets of pages. The first set presents the methods used in science (this would take us a little too far afield); the second, the disciplines that have come into being in science; and the third, the concepts. The next logical step is either first to examine the coordinate of levels in some further degree—we have in effect been examining this coordinate by the outline before us—or to jump to the other extreme and start looking at some actual items in the tables and from there work back up again. In the course of doing so, the application of our outline will become clearer, anything wrong can be made right, and further insight will appear. I would like to spend some time now at a highly particular level and apply these headings in detail.

GODDARD: I don't know whether I dare throw out another generalization, because I very much want to do what you want to do. Is it worthwhile for us to consider whether or not the present six items include something that we discussed yesterday and which Schmitt mentioned a moment ago in another way? That is the origin, in an ordered system, of structural and functional properties not predictable upon the basis of the knowledge of the properties of the individual units of that system. To give you an example, nobody can at present predict the properties of a protein molecule from knowledge of the constituent amino acids. The properties of an enzyme are not predictable merely from what we know of the amino acids that make it up. I don't think the properties of memory are at present predictable on our knowledge of the anatomy of the central nervous system and the properties of the individual components. Now is this already subsumed under what we have said?

GERARD: You said two different things, in violent conflict with each other. About half of your statements said something is not predictable; and the other half said something is not *at present* predictable. This is, of course, the reductionist argument, and I doubt if we should go into it. You see, to say it is not at present possible to predict is undebatable; to say that it is not possible (at all) would be a philosophical position that is hotly debatable.

GODDARD: All right, Gerard, I would like

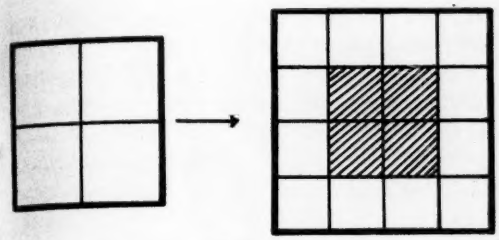

FIG. 1.

to state this empirically, not in a philosophical sense. I am not trying to make any predictions as to whether the field can be completely predicted from the parts. At the moment we cannot reach that. Now is this just implicit and doesn't need to be stated?

WEISS: [Illustrating by a series of diagrams on the board. See Figure 1]. You have a system of boxes here, with properties that can be described. Let's assume these are cells in interaction with an environment from which they are separated by a common boundary in which each of them has a share. Now let each one of these undergo further subdivision. There is a new property emerging now suddenly in the center of this system; these inner boxes have an entirely new relation to the environment because now there are four entirely on the inside which are completely cut off from the environment. This is an emerging condition, a group property, and this is an example of what we mean when we speak of emergent group properties. Obviously "inner" and "outer" don't make sense if you have only one unit or even two units; you have to get to a certain number before you can speak of inner and outer units. These things are group phenomena. This happens even in physical chemistry.

MILLER: Could we add under hierarchization another principle, which is that populations have group properties? To me, the notion of hierarchization is not simply an ordering, but includes the notion that the next larger system has a form of organization different from the subsystem.

GERARD: I think that is where this whole issue is, in the hierarchies themselves. What always happens is that, when you know the complex, you see how you could have predicted it; but, out of the almost infinite number of initial possible permutations, the chance that you would have picked that one for working out a prediction is vanishingly small. So one must directly study the complex system; that is why we have the various levels of science developing together instead of waiting for physics to give us all the answers.

GODDARD: That's right, an engineer works as an engineer, not as an atomic physicist.

[After lunch, discussion of the outline on the board was continued by Dr. Brink.]

BRINK: I am trying to get my own ideas straight as to what kind of generalizations these are. With other generalizations that occur in science, and perhaps a subsidiary set of rules of procedure, one can go back to the instances that suggested the generalization. Could I start with these, and then have some rules of procedure which would enable me to get back to the individual experiences of the people around this table, and reconstitute those instances? You see that's not predictive, just seeing whether the things that suggested this set of generalizations are in there, in some sense. It's not clear that I can do this and, therefore, I am not sure that this is the basic set of generalizations. But if it is a basic set of generalizations, what must be added to it in the way of rules of procedure? Without these formal symbolisms, and rules for manipulating the symbols, it doesn't seem to me that you can work backwards to the instances that suggested the general principle.

WRIGHT: Biology just isn't at that level.

GODDARD: There are instances that you can; genetics for example.

WRIGHT: Genetics, that's an isolated field.

BRINK: With number III, on open systems, you can go some distance.

WRIGHT: But in genetics I can have one gene that will put a rosette on the forehead of a guinea pig. A second gene will produce a slightly different rosette. Put those together, you ought to get a stronger rosette; instead, they cancel each other—in other

cases, however, gene effects are additive. There is no way of predicting in advance, with our present state of knowledge. In just a mere combination of genes the result is probably never quite predictable, and in many cases it's wildly unpredictable.

In the color factors in the guinea pig, there are something over a million different combinations that can be made. I have made what seemed to be the more significant ones, and I doubt if there are any cases where the effects are really additive.

BRINK: There are rules for manipulating properties of open systems—you would have to add subsidiary statements in order to do this, but you could. And even there the question is how could one get back to deducing individual instances of properties that suggested the third statement. Of course, in this case we could proceed a little better with simple types of experiments that lead to the notion of an open system. But if, as you say, in another instance you can't do this, then what kind of generalizations are these as compared with the sets in other sciences?

Within the system of thermodynamics, for example, if you write down a certain chemical reaction which you have determined by using chemical means and then want to discuss it in terms of the rules of thermodynamics, there are definite procedural rules by which you can do this.

In other words, with each of these there must be a set of procedural rules by which you get back to the instances that suggested these generalizations.

GODDARD: But these are very unequal. For example, I am sure that Mayr, Wright, and the rest of them could formulate the more specific statement of No. VI. However, while the concept of the species helps give you No. I, the fact that No. I exists doesn't tell you that there is even going to be a species. Nor does it tell you that species belong to larger groups, genera, etc. It doesn't tell you anything except that there is a hierarchy.

WRIGHT: The integrating principles of each step from gene to cell, cell to organism, organism to species, species to biota are about as different as they could possibly be. All that they have in common is that they are integrating principles making possible an entity, composed of parts, which persist in the face of changing environments, and so on, partly because of the persistent properties of the parts (they are organisms), and partly because of the specific interactions of a regulatory character between the parts—this three-body problem Weiss was talking about. At every level you have those general principles. But the actual integrating principles are just about as different at those levels as they can possibly be.

BRINK: These integrating principles, the way you are using them, are the rules for reasoning to which I refer.

SCHMITT: I would like to point out, Brink, that the periodic arrangement of the elements and the way the chemical elements are composed is consistent with the laws of energetics, but not obviously deducible therefrom. I think the law that we deduced here is that biological entities create, maintain, and change some kinds of order. This is correct, but it by no means tells us anything, in fact, about the *way* it is done. Whether the order be the thing I was discussing before or the order of a species, or of a population, is neither here nor there, as far as this definition is concerned.

WEISS: Referring to Wright's remark, the transition from cell to organism, from organism to group, is very much like the isomorphism between hydrodynamics and electrodynamics. Although the one deals with water and the other with electricity, you can apply the same formal rules—rules of behavior, so to speak—to both of them interchangeably.

WRIGHT: I think it's not as close as that. In one case you have assembled genes from the entire species into one entity, the cell; and in the other, the whole organism traces to a single cell. In the case of the group, of course, there is nothing but evolution that causes a large number of species of utterly different histories, carnivores and herbivores and so on, to form an integrated system that has self-perpetuating properties.

GODDARD: Brink has really brought something out here, if we think about it for a moment. Newton's laws of motion defined

in a real sense the relations between particles. We have not attempted to formulate in these generalizations the actual relations between organisms or between parts of organisms. Now, to some extent in limited areas of biology I think we can define relationships rather precisely. For me, perhaps the easiest cases to think of are in genetics, but I think the physiologist—the sensory physiologists, the muscle physiologist and the embryologist can formulate in restricted areas a rule that shows the relationship between A and B.

When you have done that, I think what Brink wants to do is possible. But what he has brought out very clearly is that we are not showing relationships in any functional sense between the various parts here. These are nothing like laws of biology, to be used for defining precise things.

HOTCHKISS: In the case of Newton's laws, thermodynamics, and so on, the material being codified was already highly conceptualized. It was, therefore, possible to recover what was put in. In the case of this table we are codifying and tabulating material which is to a good extent empirical. In just those cases that Goddard brought out—genetics and so on—there is a degree of conceptualization and it is easier to get back to the original. Perhaps you are asking too much and are tending to infer that the fault may lie with the table, where the weakness is in the nature of new materials that we have to put into it.

BRINK: Well, then, I would say, in connection with the discussion this morning, that we should be very careful how we use this table.

GERARD: Note that we are now examining these coordinate systems in detail. I would like to follow up Wright's statement that, while the hierarchical relationship exists, the exact procedures in each case are utterly different. Now I agree that it is just as important to examine the ways in which instances are different as it is to see that basically the same kinds of mechanisms are involved. But I doubt that analysis would show that they really are all utterly different.

WRIGHT: The very fact you've got an entity composed of sub-entities means that the persistence of the whole rests in part on the persistence of the components, in part on their interactions. You've got this interplay between persistence of parts and interaction of parts at every level, but beyond those formal resemblances there's practically nothing.

GERARD: What are some of the differences that seem important then?

WRIGHT: Well, we have spoken of composition, the relation by origin of the components to the total. The single cell contains an array of genes that a few generations before was distributed among many millions of separate individuals. The individual comes from a single cell by a process of differentiation of descendants of that cell.

In the species you've got, of course, sexual reproduction as the integrating principle. Cells are coming together, separating, coming together, forming a network in time-space, so that anything that happens in one part of the network can be recombined, if it's a Mendelian mutation, with anything that happens anywhere else. The whole network functions as a unit.

Of course, in the biota you don't even have this connection in space-time. You have organisms that have utterly separate histories, and may have come from opposite sides of the world, which somehow or other form an integrated system in which each depends little or much on others. Each level has to be considered by itself, although it is useful to make this kind of a diagram as a substrate for raising these problems and talking about them. It's a framework—not a generalization, but just cutting up of the field in a convenient way.

GERARD: How relevant is that to your question, Brink?

BRINK: On this basis the table is not a system of reasoning about the instances—it's just to remind you of what you want to talk about.

WRIGHT: It is organized—it's easy to show what the relation of one part of the field is to another, when you've got your two-dimensional diagram on the board. It's a convenient teaching device.

GERARD: Are you willing to give up all predictive value?

WRIGHT: Pretty much. There are certain

very broad aspects, perhaps—but you don't need to predict them because you know them already.

GERARD: I think you are too negative on prediction. It is true that you know most of the things you predict; but in an area that you do not know, it is often possible to make some pretty important general predictions.

WRIGHT: Well, if you can carry ideas that develop in one field to another it will be useful as an analogy, but it is not predictive.

WEISS: I think we are at a very important road fork here. If I understand it, the question is whether this is a scheme of conceptualization or a mere classification. If the former we might expect to get more out of it than we put in—it has some economic function. If it is only a classification, it is merely a circumscription. But even if it is only classification, this scheme might prevent false, erroneous classifications, which arise frequently in biology to lead people off at a tangent. Keeping a certain order in our minds, even though it isn't conceptualization, eliminates a large margin of error which otherwise would creep in.

BRINK: In any new instance in which you do an experiment or have any other experience, you could always ask yourself, "Does this fit in here? Or is this something really new?" It has that possible function.

MAYR: I feel very strongly, with Weiss, that this prevention of unbalanced viewpoint is perhaps the strongest recommendation of some such scheme. A person might be concerned, for example, only with III and completely forget that there are also II, IV, and V.

[Discussion at this point digressed briefly to a consideration of homing and thence to categories of behavior. The Chairman then introduced the notion of dimensional analysis as follows.]

GERARD: It has been suggested that this kind of a scheme plays a role comparable to that of dimensional analysis in the physical sciences—perhaps you wanted to speak of that Miller?

MILLER: Perhaps I did. Dimensional analysis just came over the hill at that moment, but I have said things about dimensional analysis in the past.

Particularly in the social sciences we are uncertain what our dimensions are, and various efforts have been made to get organization out of this, factor analysis being a primary example of that sort of thing. There are ways mathematically to determine dimensionalities of domains, and while we will find it difficult to come to agreement as to what dimensions we are going to use, certainly it would be desirable to have the dimensions congruous. Also, if we accept the notion of hierarchization and generalities across levels, it would be desirable for the dimensions employed at lower levels to be in some way congruous or translatable into those employed at the higher levels.

WRIGHT: I don't see where this has anything to do with the dimensions of energy, of action, and so on in physics. The dimensions are purely taxonomic here, a device for description.

MILLER: To a degree. However, you have decided on an order of hierarchy which is roughly related to size in space—the systems are larger in space in one direction, and smaller in space in the other; also, in general, longer in duration and time in one direction. Those are physical dimensions that determine your hierarchy to a degree—isn't that right?

WRIGHT: Hierarchization is just composition—A is composed of B, B of C, and C of D. I just don't see any connection with energy, action, and so on. It is just a sort of a pun to speak of dimension in a classification of this sort as comparable to dimensions of a cube or of energy or any of those things.

SCHMITT: Shouldn't one refer to factors rather than parameters?

WRIGHT: Factor analysis in biology is all right. You could speak of a number of dimensions of the mind, if you really could analyze it into just so many factors. It's conceivable that you might have a certain number of dimensions there, but that has nothing to do with this, it seems to me.

MILLER: May I ask, Gerard, exactly what you think is relevant in this discussion, because there are various things that could be said?

GERARD: The question is whether a framework like this serves the role in clarifying

concepts, as dimensional analysis of units (to c.g.s. elements) does in the physical sciences.

MILLER: Wright in my opinion was right in saying that we arrived at this hierarchization by logical relationships, first of all. I do think, though, that those logical relationships turn out, when you investigate them, to imply certain things about size, along both spatial and temporal dimensions, in the Chinese-box type of hierarchy. We are talking about a hierarchy of real systems which exist in space-time. You could have Chinese-box hierarchies of inclusive conceptual notions, which are more and more inclusive, yet the things they deal with don't necessarily get bigger, or necessarily live longer.

MAYR: There is another viewpoint or scheme of classification which is very useful and was mentioned yesterday, but has not yet been incorporated in our system. I am referring to the classification of causes in biology into proximate and ultimate factors and to the confusion resulting from failing to make this distinction. In almost any nature history phenomenon, like bird migration, you have different ultimate or proximate factors—proximate factors being something purely physiological that makes a given individual under certain circumstances go, while the ultimate factor is natural selection creating a genetic system which then responds to these proximate factors. Should we discuss this and does it belong in this system?

WEISS: The term "adaptation" includes that?

WRIGHT: We have that in every square here that deals with process and not description. You have immediate, ultimate factors, and. . . .

MAYR: That has been one cause of confusion in the past. The physiologist, quite obviously, tends to see first the proximate factors, while, let's say, the evolutionist thinks and talks about ultimate factors. Statements then come out that look like they were opposing each other, while actually there is no conflict whatsoever.

WRIGHT: Well, you have a column devoted to homeostasis which deals with immediate factors, and a column of history goes back to as ultimate factors as you can get at, the very nature of history. So it seems to me that you do have places there.

BRINK: I was just thinking, by analogy again, that this is the kind of discussion I would like to hear, because it serves the same function that the additional statements do in physical theories. For example, the laws of thermodynamics are not enough to operate with—you have to mention, and very usefully, the idea of extensive and intensive properties. They are all properties, but you classify them into two groups for special reasons, and then you make up rules of addition and so on. This sounded to me like something along those lines. These are additional rules of reasoning that you have to write down in order to begin to work with this table.

WRIGHT: I think it's just more classification. I don't think we ought to kid ourselves.

MILLER: We all agree that the biological systems are in space and time. We agree that the physicists have been able to use centimeters, grams, seconds and derivatives to describe physical phenomena, though there have been times when the relationship wasn't known at first. Now, is there something about biological phenomena which requires that other types of dimensionalities should be employed beside the intensive and extensive dimensions of the physical sciences?

WEISS: Well, the historical factor. . . .

MILLER: Time, isn't it?

WEISS: Well, no; it's accumulation of time, the residues of past experience. A cathedral, for instance, as a material edifice is an object of the physical sciences and, whether it be Gothic or Romanesque, the principle of its construction has to obey the same physical laws of mechanics, otherwise it would collapse. But the peculiar modes or fashions according to which these principles have been employed to build actual buildings, through an evolutionary sequence of styles, is an additional criterion in architecture, isn't it, which is over and above what you would have in the purely physical science of structural engineering? This historical element which you have to add makes the dif-

ference between structural engineering and architecture.

MAYR: Well, I feel quite strongly that way. You have in the historical way of approach something which you do not normally have in physical sciences.

GERARD: As you said to me last night, one difference between the cellular and the species levels is that structure is much less important and history much more so at the species level.

MAYR: As you go up the hierarchy—from the lowest to the highest level—there is a tendency for structure to become less important and history to become more so, if you define structure as a space relation. The word structure, unfortunately, has been somewhat elastic but the structure of population is not the same as the structure of a DNA molecule helix or double helix, or the morphological structure of an insect. About the use of the word structure in the same sense for population structure, I have always had considerable misgivings.

WRIGHT: But it is very simple. You have centers of density, centers of sparsity, and there is a certain structure of the species. At a still higher level you have rather diverse species that enter into this.

GERARD: We are now engaged, usefully despite the free wheeling, in examining these categories. A value of such a table is clearly in pointing them up for attention. Now, let's face the two terms, history and structure, which we found useful and see if we are really talking about something or if we are just throwing words around.

WRIGHT: That's a false antithesis, it seems to me. You have a description of history, and a dynamics of history—if I dare use the word—also a description of the current status—and the dynamics of that is homeostasis.

MAYR: History, in the physical world, is of comparatively little importance—elements are the same, regardless of their past history. As you go through the levels of integration it becomes more and more significant and, if you deal with a species, its adaptation, its structure, its physiology, its behavior are understandable almost entirely only in terms of its evolutionary history. That is what I was talking about, that the relative importance of the two things shifts as you go up.

WRIGHT: In the diagram I've used, I have columns for history and its dynamics and for reproduction—and they have this inverse relation pretty definitely. That is, you have under reproduction, gene duplication, cell division, physiology of reproduction of individuals, and then it peters out. You don't have any reproduction of species except in the rather sketchy sense of splitting of species, which is on quite a different basis from the cleavage of an amoeba. History goes just the opposite. You don't have very much of a history of the genes. We'd like to know exactly how a gene does duplicate but it's certainly not comparable to an evolutionary history. So that you have this inverse relation: (1) the duplication, multiplication, of a certain type of entity which is very important at the lower level of the hierarchy and simply doesn't exist at the higher levels; and (2) history, which is rather meaningless at the lowest level, which is clearly present in cell differentiation, in morphogenesis and histogenesis, in population genetics, but which gets more important at the evolutionary level and in relation to biota. Up there it is actually paleontology because paleontology is not really concerned with species, except by inference. It is concerned with the succession of whole systems of life as revealed in the successive strata.

SIMPSON: We deal with species in about the same way and to about the same extent as the neontologist usually does—not the geneticist, but the systematist.

WRIGHT: Perhaps it is the most important means of deducing phylogeny, in deducing the history of species, the most direct evidence but it doesn't give you. . . . There's a lot of deduction. You are actually given the fauna and flora that existed at the time of a certain stratum, so that in paleontology your field really is the history of whole systems of life, it seems to me.

SIMPSON: I'll accept that as an ultimate goal of our field, but the fact is that we do deal with species in just the same way that the student of recent faunas does.

WRIGHT: I'd say you deal with the evolu-

tion of species more directly than anybody else, but even so it isn't really very direct, the thing you deal with most directly is the succession of fauna and flora.

SIMPSON: Well, no systematist of recent animals is dealing directly with the species either.

WEISS: I don't see that a cell has less of the history of the species incorporated in it than has the species as a whole. Whatever happens in the individual cell has to be balanced, it's a complex system; the balance is adaptive and has been established as part of the evolutionary history. I don't see how you can make quantitative distinctions.

MAYR: I was thinking of components below this. The cell, since it has a chromosome, is a part of the species in that sense. But I'm talking now about, let's say, Schmitt's macromolecules and things like that. On that level I think history is very definitely less important.

WEISS: Oh, no! That's where I think it is simply a question of facts because you wouldn't have this particular property of the collagen unless it were produced by a cellular machine. You can't isolate it from the conditions under which it is produced and that's why I think structure should not be conceived as a space pattern, but as the structural processes that antecede the space pattern.

GERARD: Mayr is not trying to say that there isn't a history to a nucleoprotein molecule, just as there is to a giraffe, but that if you know the history of the giraffe you can understand why it is the way it is to a much greater extent than you can understand the nucleoprotein molecule in terms of its history.

MILLER: Can we make a generalization that the more complex the system is the more important the history is, because it involves a series of choices concerning alternate organizations? So there is more to the history of a skyscraper than a hut, and more to the hut than to a tombstone. This is the only thing that we are really talking about.

There are certain facts about the history of the very simplest systems which are important, for instance, only sodium and chlorine can give rise to sodium chloride. There is a principle, even at that level, that things arise only from similar antecedents. The more complex structures have layers and layers of development of this complexity; this is the history which interests us.

SCHMITT: I return again to the realistic value of all this business. It isn't nearly so important to bring up individual points and say, "Does this fit into it or doesn't it?" as to say, "What are we going to do about it?"

For example, we take this question No. I under hierarchies and we say "entities are populations." If we say that the chromosome is a system of physical and chemical entities, this is useful. If we come to macromolecules of the kind I've been talking about there and say that they are populations, this is unquestionably correct. The physicist would see this array of entities which themselves have populations within them; what then will he say which wouldn't occur to us as biologists? To what will he extrapolate and come out with a meaningful experiment? What I have done is to hypothesize from what I know about certain macromolecules and made a big jump and said chromosomes are like this. That may be useful and it comes strictly out of this kind of thinking.

Now, maybe we can find at every level here ways of doing that. To me that would be very useful.

BRINK: This comes close to what I had in mind earlier, because you are saying that you look for some way to make this come alive, rules of procedure for reasoning about. This is what an analytical mind naturally seeks. If you want to stir up future physicists, then it's the rules of reasoning, not just these categories, that will attract him.

HOTCHKISS: I rather think that the discussion of history has been a little unfortunate, perhaps only through use of the particular word. I thought it was a principle of biochemistry that there is a genuine history at the molecular level implicit in structure and the particular kinetic states of the molecules. DNA is a dramatic instance because it is a translatable kind of history. The hierarchical steps from the molecule to

the society go, on the one hand, from things so small that they have to be dealt with statistically, to things so big and complex that we can only get perspective by viewing them as statistical units in a big group. What we have in between are things just close enough to man's size that he can see them with a hand lens and deal with them not quite so statistically. I think our statement about the uses of history is a little too broad and perhaps some of the difficulty is at the in-between level—we don't easily find ourselves dealing with these populations with statistic perspective.

MAYR: This is the main difficulty in the history of evolution and is why, for instance, the German biologists, between 1830 and 1859 had pretty well missed out on the problem of evolution—because they were typologists. The English, coming from a different philosophy—Locke and so on—of which statistical thinking was part, came to the right solution simultaneously, as Wallace and Darwin. So I am not so sure that statistical thinking is negligible at that level; I think it is important at any level.

WEISS: The study of development is one of these intermediate dimensions where we can show that the necessity for a statistical or stochastic consideration, as against micromechanical precision, is not a result of the inadequacy of our tools or the errors of observation, but is a truly inherent property of living systems.

MILLER: Statistics because a phenomenon is too small; statistics because it's too large—those are two entirely different reasons for using statistics. The first is because you cannot follow the path of something which is too small except by finding its statistical effect on surrounding bodies; the second, because you cannot study the total population at once—it would be exhausting.

WEISS: There's a third reason, Miller—the positive one. It isn't that we can't study the pathway but that where we do study it, we find it unique in each case. It still leads up to what you call equifinality; it implies the feature of non-identity, yet sufficient constancy to make us speak of individuality.

MAYR: All biological phenomena are basically population phenomena.

MILLER: Aren't there two primary sorts of statistics? Descriptive statistics deal with things like variability, for example; sampling statistics are used simply because it would be exhausting to study each member of the total population, or impossible for some other reason.

GODDARD: Though I'm very much opposed to typological thinking, and recognize the statistical variation of organisms, I still think that we must as biologists recognize that we do deal with individual organisms. We grant that the organism itself is made up of a population. But one can work with a fly and not be concerned with the fact that it is made up either of cells or of molecules. The individuality of the organism is not wiped out by the existence of either populations or statistical variability.

WRIGHT: But you haven't any science except as you deal with classes. A fact about an individual, unless you can generalize from that in some direction, is hardly a fact of science.

[Drs. Goddard, Reynolds, Mayr, and Caspari exchanged views on the role of the individual in biological observation and research. It was generally agreed that the individual is utilized as a sample of a population. The discussion continued with a statement by Mayr on the issue of typological versus population thinking, as follows.]

MAYR: The older philosophies were all typological, going back to Plato's eidos, etc.—all variations were merely shadows on the cave walls of the eidos and the eidos was the proper thing. This, when translated into natural science, led to establishing the norm or type, and everything else was something to be ignored. Population thinking is to assume there is no such thing as a type or a norm, only a statistical mean or a mode. You're dealing with a variable phenomenon which is variable for a good many reasons.

In modern physics this approach is coming in more and more, although in physics it is less of a necessity than it is in biology, where the individuality of the individual and of the species and so on is so much more pronounced. I believe that this gradual displacement of typological thinking by population thinking in the last century has been

one of the most profound intellectual and conceptual revolutions in biology.

GERARD: How does this apply to Caspari's statement that a physiologist analyzes one frog sartorius muscle in the hope that this represents a pretty good sample of frog sartorius muscle in general, a less good sample of frog muscle in general, not too impossible a sample of skeletal muscle in general, and probably a pretty poor one of muscle in general?

BRINK: I think that a physiologist, in the cases you just mentioned, is figuring there is a population distribution and in picking a few samples of it he's picking representative members. The fact that he averages to get a mean value makes him feel that he is probably somewhere near the center of this supposed population of frog muscles. He doesn't worry too much about whether he's got the prototype of all frog muscles.

WEISS: Could I make an additional comment here? The physiologist will pick his muscles for a particular purpose—determining contractility or excitability perhaps—and he will choose his sample very close to the mode with regard to that property. Now, the same group of muscles would have an entirely different type of distribution with regard to fiber size or the thickness of the connective tissue. It wouldn't be anywhere near the mode. So again, there is no "normal" muscle, as such; one muscle may be normal with regard to excitability and another muscle would be normal with regard to anatomy.

I want to say that, unfortunately, medicine has not profited from this type of thinking; it is deep in typological thinking. If the sort of thing we are doing here in biology could penetrate into pathology, it would have a tremendous effect in clearing the minds of men in medicine for more realistic approaches to the problems of disease and "abnormalities."

MAYR: As many physiologists say, "*The* frog does such-and-such." Now, there is typological thinking.

GERARD: Yes, but to say "*the* fly" is merely to state, in effect, a mode or an average. This hasn't been clearly recognized and it is, therefore, better to say that there is a certain probability distribution and that the chance of getting this particular value out of the whole population would be so-and-so. But it doesn't seem to me that this is an utterly different way of looking at it.

SPUHLER: A case from the history of evolutionary thought is very appropriate here because if you think of the individual as evolving you can't even begin to conceive of the mechanism that you conceive of if you think not of individuals but of populations as evolving.

MAYR: Typologists, in evolutionary theory, are all saltationists in their thinking. They had to be saltationists because there is a discontinuity, a bridgeless gap as Goldschmidt would call it, between species. But, if you are thinking in populations, the tails of your curves already overlap and it is very easy to shift over. So in evolutionary thinking, going from typological to population thinking is a very drastic and important shift in concepts.

GODDARD: I would really like to make one remark if I could. I have no quarrel with Mayr on the matter of population thinking of this sort. I realize the dangers that come up when you try to make the average, for example, that was mathematically derived instead of statistically expressed, as the midpoint. You will find that in modern handbooks in physiology and so on, you very rarely find normal values given any more. You find, for example, that from this end to that end 95% of the values are included and 5% will be outside. We need more of this. But the individual is a real event; the individual is, in fact, observed and studied. By studying a series of individuals you improve and extend the knowledge, but to deny the existence of the individuals!

WEISS: But who does, Goddard? This isn't what we've been talking about.

MAYR: I think it can be rephrased to say that when we are dealing with collective phenomena, they are of the population type. Just take the concept of race. Looked at typologically, how insidious, how pernicious it is! Look at it from the population viewpoint, and it is merely a naturalistic description of something in nature.

[The consideration of typological versus population thinking, and the role of the individual in biology, continued for some minutes in very much the same vein. The Chairman then directed discussion back to Dr. Schmitt's concern over the value of the whole effort.]

GERARD: You were asking yet again about the use of this activity. Several levels of use might be worth looking at. The simplest is to have biology put out, not as a series of utterly unrelated factual statements or even unrelated concepts, but to start with a table that says, "When we are talking about this area it has this kind of a logical relation to the other areas." This is a real gain.

A further gain can be obtained by using a more active process of teaching. Present this kind of a scheme to a student, or a physicist, or even a colleague who isn't used to thinking with it, and very little happens. But if, in the course of leading a discussion with your students, you get them to come up with a scheme (and you'd be surprised how regularly it would be the same scheme in essence) it becomes part of their thinking. They know how they got there even though they can't retrace the steps; they live a bit of scientific thinking; and they really change their orientation toward biology.

The last step and the greatest gain, if you achieve it, is the one mentioned earlier concerning predictive value. If certain things are grasped at one level, one may be encouraged to predict, or look for, or apply, the same sort of thinking at another level; and a certain number of times it will pay off.

I think these are all perfectly valid gains to expect from this approach. How much can be realized in any particular case depends on the situation in which one is able to use it.

SCHMITT: I think, in respect to the teaching value, whether to high school students, to Ph.D. candidates, or to colleagues, we have to bear in mind the type of intellects with which we deal and the things which fascinate those intellects.

It seems to me that, by and large, biologists think along two lines; analytical and evolutionary—biophysical, biochemical, and physiological or evolutionary, systematic, ecological, and so on. If we could only put forward the great challenges which lie in each of these general areas! Unfortunately we have our individual "union" rules and we sit around this table and we debate on the basis of the union in which we were brought up.

I think this kind of thing has predictive value. I could illustrate by saying that not only do macromolecules sort out their own kind and precipitate with specific patterns, but I've long thought, and I'm sure Weiss agrees with me, that cells find their own patterns for exactly the same reason. This could be tested experimentally. But, on the whole, it seems to me that this conference on which we're spending so many man hours is going to be of greatest value in attracting people into the field, not in our debating amongst ourselves.

[The Chairman, in outlining the work for the remainder of the afternoon, called for specific suggestions, to be voted on by the group, suggesting on his own behalf that attention turn to the details of the outline. The suggestions follow.]

MAYR: To continue with the framework, or with this item "order," "ordering" or "formation of order." I feel that we have here merely a sort of a frame of reference and if we want to arrive at something a little more useful, we might discuss some of the things under one of these items, and see what all these subitems mean and how they fit in. I would like to single out ordering for special attention because at the evolutionary level we have a number of published treatises that try to synthesize and classify data, while on the level of ordering, we have much less in the line of comprehensive accounts. We have here several people, like Schmitt, Brink, and Weiss, who have been giving thought to this, and I thought it would be very fruitful if we could discuss that particular heading in more detail.

BRINK: By way of picking up some information for myself, to discuss what is the present status of the relation between DNA molecules, genes, gene statistics, and molecular mechanical statistics. But I would have almost nothing to contribute to this.

MILLER: I think we should be pretty particular from here on, deal with some of these specific suggestions, at different parts of the hierarchy.

WRIGHT: It seems as though you are asking here for about half a dozen textbooks.

GERARD: I judge this is not a suggestion.

WRIGHT: No, it's not a suggestion, but if you're really going to take up these specific matters, you will have to write a textbook on each aspect.

GERARD: Maybe it would be useful to see what we could do with the first chapter of one of the textbooks.

WEISS: I would very much like to hear Mayr and perhaps Miller indicate how much pragmatic value they think there is in following up the isomorphism between the morphology of a flock of starlings on the one hand and, let's say, cell behavior or the morphogenesis of a slime mold colony on the other. I'd like to see that type of thing carried through to see whether there is really some concrete value to be gained.

GERARD: Isn't this really asking for further discussion under the topic of ordering, just as Mayr said?

WEISS: I wanted to make this point as a practical example of one hierarchical principle that one could, really, carry through from the top down.

GERARD: Are there any other suggestions? We have a specific request, and a subhead under it, that we go next to the topic "ordering," and that we explicitly examine the meaning of it at the different vertical levels, especially the group and population levels. We have an extremely specific suggestion that we examine the relation of DNA to genes, partly an ordering. We have a general suggestion that we get to the particular, and a particular suggestion that we get to the particular, and start looking at materials in the document you have.

SCHMITT: Mr. Chairman, I have still another alternative suggestion which you may rule out immediately. It would be interesting to me to go around the table and have each conferee describe the concepts he is most excited about, because I wonder to what extent that would make this whole discussion come alive.

GERARD: If you don't mind, I will rule you out of order.

SCHMITT: I was sure you would.

[The group voted to discuss "order."]

GERARD: Mayr, take over, we will discuss "order."

MAYR: I was posed a very specific problem by Weiss and, frankly speaking, it's the sort of thing which has never particularly excited me. Somehow or other I have never seen anything particularly useful in comparing phenomena so far apart as a vacuole in a cell and the vacuole around a bird of prey.

WEISS: That's why I introduced the whole morphology of the starling cloud, which has a clearly delimited boundary-like interface and retains its shape in spite of the changing positions of the individual birds. A school of fish could do the same thing. I'm asking about that type of phenomena. The thing is, when a flock is divided, there emerge two clouds, both of which now are spherical, and this type of phenomenon is quite analogous to twinning of embryos or slime mold colonies, although of course on a simpler scale.

MAYR: Yes, but it's hard to get away from formalism and get into some causal analysis. The difficulty is that so far no attempt to study the phenomenon of the starling flock has been very successful.

MILLER: May I say a couple of things about this? I remember, about last May, Simpson and I had a long discussion, and I think the thing that was perhaps least convincing to him about the notion that systems at different levels were comparable was when you got out into the multiorganism systems; where the boundaries are not physical boundaries you wonder whether there is any real similarity between this ordering and the ordering of a multicellular organism.

WEISS: Is it all right to discuss this point? I think the boundary of a starling cloud is exactly as sharp as is that of a cell, because the distance between two individual birds, relative to the mass of the bird, is no greater than the size of the pores between the macromolecules of the membrane.

MILLER: I have thought about this in

terms of, say, a naval fleet at sea, where you have a similar relation of ships to each other as compared with the total environment. As long as they can communicate or send information back and forth to each other, then they can turn at a given moment, they do not run into each other, and so on. When a typhoon stops communication and information can not get across, then the problem is how you keep one of these components of the total system from slamming into another. This is very much like a school of fish, which you may see suddenly turn and almost instantaneously go in the opposite direction. Obviously, schools of fish could not behave that way and remain intact if their sense organs were not operating. Perhaps there have been experiments on such phenomena.

I wonder if we can analyze the difference between such situations as maintenance of organization of the school of fish or the starlings; coordination by the pituitary of a number of glands, quite far apart in terms of average cell size, by messages conveyed chemically; or integration of parts of the autonomic nervous system by messages sent electrochemically along the neurone, and then transmitted neurohumorally across the synapse. These seem, for all their differences, in important ways like coordination by smell or taste among some species, or whatever it is that coordinates myxamoebae. These phenomena impel conviction that the formal identity between individual organisms or systems and ecological systems or groups is more than superficial analogy, for they all involve information transmission mechanisms, either in a multicellular organism which is all within one continuous boundary, or within a multiorganismal group, all within a single space-time boundary. The question which has been raised as to whether gene pools are systems may be settled by determining whether there is any sort of mechanism to convey information to the different gene puddles which make up the gene pools, so that they interrelate or covary. If mating occurs, or potentially can occur, then the puddles constitute a system. But if you have factors—physical, emotional, or cultural—which prevent various races from mixing genes, or various social strata from mating, then the whole gene pool may not be a single system. Social or informational barriers to mating may be as effective as actual physical barriers.

WRIGHT: The very essence is the continuity of crossbreeding in a species. As soon as that continuity is broken—perhaps in terms of hundreds of thousands of years instead of just minutes—then it is not a superorganism.

CASPARI: I would ask whether we are most interested here in the analogies or whether we are most interested in the differences. I find, when you bring these things up, the question which interests me most likely is: How do they communicate? Do starlings communicate with each other in a different way than. . . .

MAYR: That's why in spite of all the feedbacks in a fleet you have a flagship; in spite of all the feedbacks in an intercom system you have certain things like a pituitary or a hypothalmus which seems to be dominating the situation. In a flock of starlings, so far, nobody has been able to find that any one bird has even a trace of a leadership there; they seem to be equivalent.

GERARD: I may be able to prove to you, though, that something like that has to happen. Here is the evidence, published in a note long ago. I was driving forty miles an hour along a country road while a flock of birds flew parallel with me at the same speed. The birds suddenly veered, as I happened to be looking, and I could see no forward displacement of any bird versus others, so that they must have all veered together, within a time interval that would be less than five milliseconds. This could not have resulted from one bird following the other; they must all have been responding to some signal, either from a leader bird or from the environment.

WEISS: The outline of the starling cloud is very sharp, nearly spherical. Now, when this is broken in two, each half almost simultaneously rounds up into a separate sphere. It isn't exact; it has almost an ameboid motion, with distortions and elongations. The remarkable thing is that despite these deformations, they cohere and retain a very sharp outline. We don't know what the communication system could be that would tell

a bird the differential equations of a smooth, spherical surface. This is the analogy I wanted to investigate because that is really what happens.

MILLER: Caspari said he is more interested in the disanalogies than in the analogies. I must say that I am equally interested in the analogies and disanalogies, and that a proper science is that which is interested in both at the same time.

MAYR: A partial answer to this comes from the study of schools of fish, where it has been shown they have an optimum retinal image, a sort of preconceived notion of how big they want to see the other things within the flock, that is a major factor in schooling. The effort to satisfy the need for this retinal pattern contributes to the formation of schools.

GERARD: Let us go back to the problem of communication. All these birds turn together within five milliseconds, so it is not a chain reaction.

WEISS: This is like the flashing of fireflies; very much the same type of phenomenon.

GERARD: I am asking about the signal that was given.

WEISS: I'm just raising this because as long as it isn't explained in some form we are up against certain principles that, if they recur in other dimensions, we needn't be disturbed so much if we don't have explanations, either.

GERARD: Caspari, would you carry on the point you were making?

CASPARI: I would like to ask the following question: Do you think that if we knew, let us say, what keeps a colony of myxamoebae together that it would give us information concerning what keeps a flock of birds together?

WEISS: Yes, it would help me because for the time being we haven't identified in any of these systems the thing that determines the morphology of the group of units.

SCHMITT: Suppose you have two particles, on the surfaces of which there are many positive and negative charges arranged in a pattern. Suppose it is at the isoelectric point where there is no formal net charge, yet there may be a strong force between these two particles which is not a coulombic force; it doesn't vary as the square of the distance. Yet one particle may be affected by another, over a fairly large distance.

GODDARD: What do you mean by "a fairly large distance?"

SCHMITT: Of the order of hundreds, perhaps even thousands of angstroms. The physicist has not yet fully explained such phenomena as far as I know.

Now if you tell a physicist, "Here is a flock of birds—they are in a very low dielectric, air; now here are some fish, they are in water of high dielectric constant and there is communication between them." Can the biologist describe the phenomenon and set up the problem in a way that is meaningful to a physicist who wants to attack it?

A physicist might immediately ask: "How can I stop or control the phenomenon?" If it is possible to shield it electrostatically and find that it still works, then it isn't electrostatic. He would test whether the phenomenon is electromagnetic and so on. You must first be aware that the phenomenon exists before you can investigate the nature of the forces involved.

MAYR: A person could, in this particular case, say: "Is this a new phenomenon or is it, as suggested by the fish flocks, simply that a fish, being a social animal, has a retinal picture so to speak, which makes him feel happy when another fish of his species is swimming a certain distance from him?" You can do something to these fish flocks which makes them scatter in all directions and as soon as the danger, or whatever that disturbing element was, is gone they regroup again at that distance.

REYNOLDS: I don't think this is an adequate explanation of the fish school, however, because it isn't only visual. Some fish you can blind and they'll still do it; even the ones that do react visually don't school with fish of the same species of a different size which presumably, if it were visual, they could do just by changing the distance.

SCHMITT: If the discovery of how bats navigate had come before we knew about radar it would have been impossible to say how they did it.

WRIGHT: How long does it take a bat to react to a wire that he is flying toward?

It must be a pretty small interval. How many microseconds does it require a bat to react?

REYNOLDS: They can catch shotgun shot, or BB-shot, in their mouths.

MAYR: I don't know whether this helps but I just need to look at a flock of starlings and can tell whether there is a hawk nearby or not because the normal moving flock of starlings is dispersed at a larger distance than when they are conscious of a hawk nearby. Then it is an exceedingly compact flock. These starlings have an ability to adjust according to various conditions.

WEISS: I only threw this in as a sort of monkey wrench, an experiment. I've been a bit disturbed that in the past few days we've talked as if we were nearing the end of biology development. We've talked largely about concepts which we *do* understand, and I thought it would be very nice to hear a little bit about the prospects, for the future, of concepts which we don't as yet understand at all.

SPUHLER: In social behavior there might be an analogy here. For example, you can make predictions whether a small spontaneous group will stay together and keep interacting, and thus have a boundary in the same sense that the flock has, by specifying the types and frequencies of interaction that's going on, on a very few variables: originating action and terminating action; asking for information and giving information; giving an order and receiving an order. In groups that break up, it has been shown that these variables have different values than for groups that keep interacting as a group.

MILLER: I've got a good deal of benefit in my own thinking out of analogies between these and electronic situations; let me mention three briefly. First, the difference between the effect of drive on locomotion and motion, and the effect of sensation. Obviously a disequilibrium (which is the drive) forces a robot rocket to jet along and then there are feedback mechanisms—sensory mechanisms—which steer it and maybe home it into a target plane.

Second is the one mentioned about the fleet which is going along as a fleet, no part running into any other, until finally the channels of communication are blocked, which is very much like putting electrostatic shields on the birds. Then, if you happen to be using electrostatic forms of communication, the individual ships run into each other.

A third analogy is based on the fact that various field phenomena that we know at molecular levels may compare to field phenomena at higher levels. These concepts are empirically test-sound.

WEISS: Isn't the common element in all this the order, the structural order, which is static? Static order is merely the resultant of ordered behavior so that the processes in back of it and the real order of behavior is something which must somehow be inherent in the elements.

MILLER: They have to be a comparable form; let me illustrate this from the fleet. Submarines have arrangements whereby, without communicating from one to another, they can weave a random course. They have a series of gears, *A* through *X*, let's say, each with a different form of curve. These gears rotate on a drive shaft every few minutes. Each ship gets a code in advance—at 12 noon you will use gear 9—and they are replaced exactly at a certain time. Thus they can maintain complete silence of communication, but they have stored information, like memories, in each of the ships.

Isn't this what happens in the gene pool? There may be a period of time when the individuals who are in the gene pool may not be communicating at all, they don't need to. Then there is a—as it were—a memory which is perhaps activated by the amount of light in a day, or by temperature, which results in these genes coming together by a mechanism which is complex but which is understood in different species. I think comparing a physical system like our submarines with, say, what it is that brings migratory species together at a certain time, is a valuable analogy, and suggests experimental methods for studying the biological phenomenon.

HOTCHKISS: Perhaps it might just complete the picture to say that I think in bacteria you have intercommunicating systems, too. There has always been a little difficulty

in defining the physiological age of a culture. You may look for toxic products accumulating, oxygen tension, or nutrients being impoverished without answering the question because, no matter what you do with the medium, you'll find that the population will reach a certain size and then somehow or other every cell in that culture seems to know what the population density is and the culture stops growing the way it did before.

CASPARI: Is it that they really stop growing or is the death rate and the multiplication in equilibrium?

HOTCHKISS: It would be hard to say but I think they are all behaving virtually alike.

SIMPSON: Well, why do you think that is communication, from one bacterium to another?

MAYR: They all get a similar cue, otherwise they wouldn't behave in a similar way.

HOTCHKISS: I don't suppose the signal originates in one bacterium and is communicated to others, but I think that they all contribute to the signal and all respond to it.

SIMPSON: I should think that independent reaction to some environmental stimulus is not communication.

GODDARD: I am not arguing against communication in bacteria because I think there are cases where it occurs. But elaborating what Simpson has said, one could have a case of apparent communication in plants that you could show is not communication by a single experiment. If you took a whole field of plants that respond to a particular day length and plot the time at which they came into flower you would be very surprised at the similarity. It's not going to all happen in one day, but if it's a genetically well-selected population the spread may be extremely small indeed. Suddenly something happens and they all flower and you see the whole field going off at once. On the other hand, if you raise one plant in isolation, in the same environment but isolated from all of the plants in the group, you'd find it flowered at the same time. In this case we happen to know what the signal is—the length of the night—a trigger mechanism that set it off. So you could apparently have communication between the plants, but actual communication between the environment and each individual plant.

WEISS: But I thought Hotchkiss said you could exclude these environmental factors.

GERARD: In a chemostat, where you really keep nutrition constant, this phenomenon doesn't appear.

MILLER: Certain birds when shown a hawk shape will dive down. That shape reversed will not cause them to dive down. This response is coded in them the way you have memory in a submarine. It may well be that the flock that Gerard saw flying along had built into their nervous systems a particular type of formal stimulus, to which they all respond in a certain way, and there may be no communication among that group at all. You must allow for that explanation.

REYNOLDS: A school of the common menhaden tends to form a long—a snakelike thing. These fish are small and therefore very numerous and as they swim along communication always seems to proceed from the head end; in other words, they'll come to some obstacle and as the school proceeds past that point it will be doing the same thing as each successive part reaches that point.

Whether or not they are communicating with one another, or reacting to an environmental stimulus, you can't very well tell. But, if a rockfish comes along, for example, and takes a snap at the back end of this school the back end will begin to jump out of the water and so on toward the front.

GERARD: It seems to me that most of the points are fairly straightforward. A whole group of individuals can behave in one way because of a simultaneously received clue, or each can communicate to the next.

Communication is one way of examining order; there can be an order in the communication channels or networks—perhaps the main basis of order in groups—without material pathways. An example has been worked out by Rapoport, particularly. He started to develop the equations for activations of successive neurones, on the assumption that they form a random net: neurone *A* connects with, say, two other neurones which it can excite, each of those (on the

second remove) with two, and so on; but soon some connections will be to neurones that are already activated, and the rate of spread of activation will tail off. Now, certainly the conduction of nerve impulses over neurones has absolutely nothing to do materially with the spread of a rumor in a population; but the phenomena are essentially alike in that there exists a state of activation or non-activation, there is all-or-none transmission, and there are certain lines of communication. One would expect, on this basis of the formal identity of the situations, that the equation would hold equally in the two cases, if the communication network is indeed random.

The spread of excitation in a group has been followed precisely. Some of the early experiments were done by the Air Force. Leaflets, telling how to get a free pound of coffee at a certain grocery store with the right password, were dropped on a small town, and the spread of this information was traced. Instead of having a constant coefficient of spread, the first knowers each told an average of about 8, as I recall, but each of the 8 who was told informed not another 8 but less than two. Well, to make a long story short, this proved to be a way of exploring social networks. A person who found the leaflet told all his immediate friends, and each of them started to tell his immediate friends; but, because this was a closed group, the chance of its getting out of that group to new contacts was relatively small. Once the spread did get out of the group, the coefficient began to rise again. So here was a quantitative study of social structure, in terms of communication channels, which originated and was developed from a study of the transmission of excitation in a neural network.

SIMPSON: I'm going to be a little extreme here, but you're saying there must be some analogical validity, because the same kind of measurement could be used. At the extreme, there is something closely similar between a rock and an orange because you can measure them both with a ruler!

GERARD: Even they have one extremely important thing in common—they are entities; even, round material entities!

SIMPSON: Well, if we are going to be as vague as that, what is the analogical value?

MAYR: What heuristic value does this particular example have? What I liked about Weiss's starling cloud, it points out something we don't sufficiently understand, and that's the sort of thing we are all excited about. Another instance, one of the ordering phenomena—we will have to do a little stretching to call it an ordering phenomenon—has to do with selection phenomena, in a population, which are advantageous to the population as a whole but which at first sight do not seem to give any selective advantage to any of the individuals. An increase of mutation rate in one population as compared to another, or an increase or decrease of the percentage of sexuality, as a whole, may give the population an optimum survival value. But increasing or decreasing this particular property in any one individual doesn't necessarily add to fitness (in the R. A. Fisher sense) of that individual. Here again is an ordering problem which in a way is very hard to understand. I, frankly, have never been able to quite understand it.

MILLER: I think it is not quite like measuring with a ruler, I think it's more like measuring two or three phenomena with a template. If you had a series of holes of different sizes for sorting oranges, and found that you could also sort a whole bunch of rocks you'd have something more comparable. Actually this occurrence Gerard talks about is quite complex and to find it repeats itself in a number of phenomena which, in addition, have obvious systematic formal character similarities, is interesting.

WEISS: Aren't we actually arguing the value of models, and didn't Brink yesterday refer to this, and isn't it clear that the value of the model is that you can treat a problem analogically, the original of which is not amenable to direct investigation? But the value of the model will not be established until you have carried the thing back to the original situation and tested and validated it there.

GERARD: I was making two points.

Simpson is worried about the one in which I was not momentarily interested. This is that, having got at structure in a population of neurones by putting a communication wave through it, no great step was required to use the same device in looking for structure in a social group.

The other point is that order is a matter of spatial relations, or temporal relations, or communication relations, or anything else that has some kind of stability. The question came up, what is the kind of communication going on between individuals in a starling flock. This illustration is an example of the direct measure of the communication structure of a social group. I don't know whether starlings communicate this way or not.

WEISS: I would like to reinforce this by one question to Brink and Schmitt, and so on. Don't you believe that quite a bit was gained from the mechanical picture of a wave in a liquid for the understanding of optics and electricity?

BRINK: I was just going to say that I think the similarity here is a little bit like certain mathematical formulation; you have an operator, differential equations with the function on which it is going to operate not specified physically. You can make solutions for it and you may find that they satisfy descriptions of sound waves, or water waves, or certain solutions even satisfy wave equations for quantum mechanics. True, you are using the same kind of an operator all the way through, and yet you certainly do something much more to identify the physical object of the physical processes that you are talking about. It's the operator (or set of relations) that is common here. Some formalism will take care of each of these cases provided you know the right physical thing to plug into the equation.

WEISS: But isn't a lot of our present biological work impoverished by the fact that people don't look for these operators? Take endocrinology—how much we know about the agents, which we can purify and extract and even synthesize, and how comparatively few people worry about their mode of action. Not until we know both do we have a complete and consistent understanding.

BRINK: The time is ripe, certainly, for this kind of thinking, and you see it much more now in applied physics, where stochastic reasoning is coming in. You have a physical problem that is too complicated to solve right head-on; you try to think up a statistical game that's like it.

SCHMITT: A good example of that is optical transforms to check on any particular theory of protein structure. You first draw a pattern which depicts your hypothesized structure, then reduce it near to the grain size of the plate; then you make a light diffraction pattern and take a picture of it. If this pattern resembles the X-ray pattern your theory is confirmed [illustrated this on blackboard].

We did something like that many years ago with paramyosin, the thing which gives this particular electron micrograph [displayed it to the group]. Bear had studied the X-ray diffraction pattern; we had electron microscope pictures of it, which showed a series of bands and also some spots on the bands. These spots happen to lie along certain diagonals, so this net was laid out in a drawing with the particular relations between lines and spots as we saw them on the electron micrograph. This was then reduced photographically as far as it would go. The little plate of glass was put in a collimated beam of light, rotated, and a diffraction pattern obtained. The light diffraction pattern proved to have significant similarities to the X-ray diffraction pattern.

This optical transform method is a short cut for much painstaking calculating on the new helical molecules. We ought to be able to do something like that in other types of biological research.

WRIGHT: In population genetics, the differential equations for the distribution of gene frequencies are essentially the same as that for Brownian motion, and I'd have saved myself a good deal of trouble in solving an integral equation—getting at it in a sort of backhanded way—if I'd known about the equation of Brownian motion.

[The Chairman proposed that the participants meet in smaller groups after the evening meal, taking the results of the deliberations as the basis of discussions on the third day. After some debate, it was decided to adopt Dr. Reynold's suggestion and consider instead the tables at the end of Dr. Gerard's paper, "Concepts and Principles of Biology." To effect this, 3 groups were appointed: "Structure"—Caspari, Goddard, Schmitt, Weiss; "Function"—Brink, Hotchkiss, Miller, Reynolds; "History"—Mayr, Simpson, Spuhler, Wright. Upon reconvening the following morning, the chairman recognized first the "History" group, whose members had prepared a one-page summary of their results. Dr. Mayr spoke for his colleagues.]

MAYR: I might say that we tried various approaches and finally found it most constructive and useful to present a series of statements, embodying what appeared to us the major generalizations on which there is essential agreement within the field of evolutionary biology, or "change of order," as we have stated it in our earlier outline.

We feel that (1) *"Development, the history of the individual and of its components, is a different kind of phenomenon from organic evolution, the history of organisms,"* regardless of the fact that ontogeny itself has, of course, evolved. But though the time factor is involved in both of these, and therefore the term history is perhaps legitimate for both, development of a given zygote and what happens there is much closer to the phenomena of ordering than to the phenomenon of organic evolution. This would, perhaps, be the first point one might stop and question.

[It was decided to read the entire group of statements, before opening the floor to debate. Dr. Mayr continued.]

The next is that (2) *"The nature and diversity of organisms are the result of the historical phases of descent with modification,"* and it occurred to me after we completed this that, in view of recent discussions that have gone on in the field of theoretical physics, initiated apparently by Niels Bohr, it might be useful here to add a statement, roughly reading *"the known evolutionary mechanisms are sufficient to account for the organic diversity in available geological time,"* because that is a thing some physicists have questioned.

The next deals with the material of evolution, and merely states that (3) *"The material of evolution is genetic variability produced by gene mutations and chromosome aberrations and enormously amplified by recombination."*

The next (4) *"Evolution is brought about by interplay of directive processes such as natural selection, recurrent mutation and immigration, stochastic processes, and occasional unique events, such as polyploidy."*

(5) *"The usual direction of evolution is adaptive, caused by natural selection."*

(6) *"The multiplication of sexual species is initiated by spatial isolation or by polyploidy. It is made irreversible by the building up of isolating mechanisms."*

(7) *"The pattern of evolution is characterized by interplay of three elements: distribution of populations into different adaptive zones; progression within an adatpive zone; and relatively rapid shifts from one adaptive zone into another."*

(8) *"Cultural evolution is a biological adaptation, with non-genetic modes of transmission, which supplements somatic evolution."*

Now, we compared this with the sets of statements in Gerard's paper and we had a feeling that it covered everything after point 6, because the first five statements dealt with developmental processes.

GERARD: I ask first for comments on the over-all collection of eight.

WEISS: They are, to me, unequal in content. Some are principles, some are condensed expressions of a certain scientific experience, and others are articles of faith.

MILLER: If our group were to get together again we could make statements of this sort in the area of function, too—perhaps a good many more. I personally believe (and we discussed this at some length in our group last night) that this sentence form, which is provable or disprovable, is the most useful and will further the purposes of this committee most. But there are other ways of doing it, such as organizing the concepts by levels. This is another issue which I would like to bring up because it is possible that a series of statements could be made which

would apply to certain levels but not all levels in contrast to these statement which, I gather, are expected to apply to all levels and all species.

WRIGHT: It deals with just one level, the evolution of species. We just don't go into other levels.

MAYR: Species and higher categories—from the population on up.

WRIGHT: Well, species and higher categories, but evolution only at the level of the species. We didn't go into the evolution of biota, and of course below that we didn't go into the history of the individual; it's not evolution, it's history within a certain level. Since none of us were embryologists, we just didn't feel that we were in a position to make statements about that.

MAYR: We finally decided, just as Miller said, that this list of concepts might be the most productive because it's something you can do something with. You can disprove it and you can discuss it, because it's concrete.

SIMPSON: I might also add that we thought of many other principles. Perhaps our main effort was in pruning, trying to get the ones that were most general. We could go on and on, obviously, getting more and more definite.

GODDARD: Actually these are the kind of statements which I had hoped, the first day, we would try to make for various areas of biology. Granted that some of the statements we came up with would either be challenged by other people or we, ourselves, on later reflection would have to refine and modify them. I have the feeling that in this sort of work it is much more important to formulate your ideas, even with errors, than not to formulate them, because the mere formulation is the flag that brings somebody up to challenge you on a particular formulation.

WEISS: Perhaps we could have another meeting in the future, and not only try to formulate, at greater leisure, some of these statements but also pick some from literature, that is, from people of wisdom who have given thought to such matters, and use those as a point of departure. I think we could use some of these things as they have appeared and focus our discussion on a few existing tenets and either confirm, deny or modify them. Then, after they have gone through this cleansing process, put them down and say, "This we believe, we stand by them."

MILLER: This set of eight sentences seems to be a valuable contribution and if it could be expanded, say as homework, before another meeting, by having each of us add as many propositions as we could and categorize them under history, function, and structure—perhaps having someone then take these and further subcategorize them—I think it would be a tremendous contribution. Then you have statements under which can be grouped empirical studies which would either confirm, disconfirm, or alter the interpretation.

HOTCHKISS: It strikes me that, if the eight points were to be extended to cover all biology, what is most lacking is perhaps just the molecular and cellular things in the lowest orders of the hierarchy. Point 1 says that the kind of history recorded in a macromolecule or a cell is really different from all the things that we are going to say next.

GODDARD: I wonder how Weiss would feel if I remarked that in one sense there is no real point in separating structure from development.

WEISS: That's what I was going to say.

MAYR: That's exactly why we took it out; it didn't belong here.

SPUHLER: I think that the word "evolution" could legitimately be applied to such things as the early evolution of genes and the development of gene lineage, but it was not done in this. . . .

GODDARD: Even if you do that, that still doesn't make embryology the same thing as evolution.

SPUHLER: No, but it does take up some of the problems at least.

MAYR: Well, we showed that the word "history" is not quite the right word; the cleavage wasn't made in quite the right place.

WRIGHT: If the committee had been differently constituted, it would have been all right. It was simply that the four of us were

not in a position to lay down general principles at those lower levels.

GODDARD: I am sure that there has been an evolution of the cell, and an evolution of sub-cellular units. I would suspect that some of these same processes have operated in part of that evolution, and there is no reason that I know to assume other processes than the ones we see here. Once you get below this, I think there can't be any intelligent discussion because there is no basis for it. You can speculate on the origin of life but you can't do what you can do for evolution because you have concrete knowledge of neither history nor mechanism.

WEISS: I agree with you absolutely, Goddard, because I think if these documents are to have any value, we must be very careful to declare clearly what is scientific fact and what is extrapolation and postulate or axiom. Unless we do this we are mixing in our articles of faith.

SCHMITT: I also agree with this absolutely; nothing could confuse a competent theoretical person more than a hodgepodge of stuff most of which is guesswork.

[The suggestion was made by Dr. Miller, but after discussion discarded, that each proposition be assigned to one person, who would then cite appropriate evidence in support of it. Attention then returned to whether these statements applied at all hierarchical levels.]

MILLER: Simpson, a little while ago, said all except two of these applied at all levels. I thought point 5 was one of those which he meant applied at all levels. Perhaps I was wrong.

SIMPSON: I didn't mean exactly all levels in the sense that we are going all the way from the molecule on up to the society, but that they apply to all organisms. We believe, at least I believe, that they apply to all organisms at the appropriate level, which is around that of the individual and the species.

WEISS: But isn't the organism still the focal instrument of evolution, having the genetic tools, so that anything that happens in the cell has to establish its durability through the instrumentality of an adaptive species response? The population has to use an individual and its breeding activities as a tool.

WRIGHT: It is a population in any case; it isn't at the level of the cell, of the individual. It's at that of the species.

GODDARD: The only remark that I wanted to make was that I feel strongly, as Weiss does, that the evolution of the cell, once it exists, is part of the evolution of the species. For example, in the Berberidaceae, which have very many alkaloids, we have a species that has no alkaloids. Now we happen to have a new population, which for some reason synthesizes an alkaloid. This is a problem of evolution of the species as much as the evolution of any physical character. In this case the evolution of the capacity for synthesizing this compound is a perfectly good part of the evolution of the species.

WEISS: One thing frequently not emphasized in teaching and expositions of this kind is the other side of the story, that is, the limited grab-bag of hard-and-fast elementary tools through which evolution has to work. Evolution cannot create new properties in the elementary units of nature; it can only choose and recombine the ones it finds. And this finite number of possibilities, built into nature from the start, must be recognized if we are to deal with structural and functional adaptation in a realistic sense. The properties of the elementary constituents and of their interactions are the limitations set to the choices evolution has. And that goes not only for development, but for the elementary physiological mechanisms as well.

SPUHLER: A less elementary thing that is not covered in this statement, and I think is no more speculative than comparative anatomy is the evolution of the genetic systems itself.

WRIGHT: That's pretty speculative. It doesn't belong here, I should say.

SPUHLER: It's a different realm but it seems to me it's a legitimate biological enterprise and an interesting one. I would like to call it a part of evolution.

WRIGHT: It's half of evolution, I suppose. Given the evolution of the cell capable of mitosis and meiosis, all the rest is fairly

simple, in principle. But that first half is something about which we can't do much but speculate.

MILLER: One of the great advantages of listing at least a few examples of the empirical evidence would be that we would then come to realize what operations indicate an adaptive mechanism or indicate natural selection.

SIMPSON: We didn't try to define terms here; I think it would be a very useful subject to define terms, specifically those two—adaptation or adaptive, and selection—which are the ones that are hard to define if it's for public usage.

SCHMITT: It seems to me that we have been preoccupied with writing captions and the headings for texts, and saying that we are going to write the text to go with it. I wonder if it wouldn't be possible to get something original out of this very carefully selected group. For example, it does me, a cell physiologist, more good to hear a clear-cut statement such as I've heard Weiss make in the field of embryology years ago as to the present status of the field as viewed through his spectacles; where he thinks the boundary lines are drawn and where the battle is long since past—so many people are fighting on a field which has long since lost its armies! It seems to me that, rather than to strive so hard to make some kind of a sophisticated statement of the content of biology which is defensible, etc., it would be much more important to find out what we think are now the real battle lines in the life sciences.

MAYR: The title of the conference, of course, didn't suggest what you say now. I think it's a very legitimate thing to have some such get together, but I'm not quite sure that this group was gotten together for that purpose.

GERARD: If we finish to our satisfaction the discussion of what is before us, we could use any remaining time in the way you suggest.

REYNOLDS: It seems to me that what Schmitt does suggest, however, could be a subject for this conference if, instead of putting down propositions which we consider the best test of present concepts of biology, we made a list of the concepts which are most in need of test, or being more actively tested.

WEISS: I agree, but I would like to hear what the others have to say on the content, first. I'd want to see this discussion carried on, but not tucked in between the discussion of content.

GERARD: We discussed at the start of the conference ways in which the outcome might be useful. It is impossible for a group to operate in a completely logical fashion. Whatever materials we now have can be examined as to shortcomings and effective use. Let us get back to the actual items before us and continue what further discussion they merit. Attention to the propositions, as such, does not eliminate discussion of what has been omitted or of what other formulations could have been made.

WEISS: This, I take it, is a sample of the sort of thing your group thinks.

WRIGHT: Well, we didn't pass on the question of whether it was worth doing; we just did something. We thought we'd better waive that question.

MAYR: As far as I am concerned, if I had a statement like this from, let's say, the cellular physiologists, I would consider it exceedingly useful. There are probably four or five major books in the field and a flock of reviews and other general papers but nothing like this. On the other hand, if a cellular physiologist became interested in the field of evolution, he would come to me and say, "Shall I believe what Goldschmidt says, what Dobzhansky says, what Simpson says, what Mayr says, or what Fisher says, and so on?" Here is something that may be very primitive, but it is a considered generalization of the field.

WRIGHT: These are not the opinions of any one of us but things that the four of us could agree on.

GERARD: Was there any serious difficulty in reaching these? Was there any disagreement?

WRIGHT: We eliminated the things we didn't agree too much on.

WEISS: Is it correct that if Goldschmidt had been here we would have had a minority

opinion on items 4 and 5? Speaking for the Biology Council, I hope we would not pass judgement by counting votes.

MAYR: I don't know of a single point that was rejected because we disagreed. They were rejected because we thought we were spelling things out in too great detail.

HOTCHKISS: Some of these last things seem to relate to the problem of predicting. Schmitt said we don't seem to go far into the future. I think he is making the familiar statement that a hypothesis isn't much good unless it has some predictive value, unless you can go all the way back to concrete systems. Brink wonders if we can get back even what we put in. From that standpoint it might be good to ask a general question: Is it *possible* in biology to make statements that will have a little predictive value, that will go a little way into the unknown? I've been wondering whether these statements do, and particularly, whether the things that were abandoned in the discussion were abandoned because they went just a little over the edge of the roof into the unknown?

WRIGHT: That's speculation.

HOTCHKISS: Not if it's a true generalization.

MAYR: As I said, every single one was rejected, so far as I know, because it carried things into too much detail.

WEISS: It's important, I think, to keep in mind that statements of this kind have a dual function. In the first place they tie into a package what we know and if this projects into the future all the better. It doesn't necessarily open new roads into the future, but it bars a lot of blind alleys, and to that extent, I think it has definite heuristic value.

REYNOLDS: This sounds like a detail, but I'm really sort of testing the system. There is one statement here that, as I understand the statement, is completely unacceptable. I wonder whether or not this is because of problems in definitions of words. This is No. 6, "the multiplication of sexual species is initiated by spatial isolation or polyploidy." My objection is that, as I read the words here, larger numbers of species are produced by either of two factors independently, by spatial isolation alone, without any other factors entering it, or by polyploidy.

WRIGHT: It's the problem of splitting, not the specific transformation or change, but the splitting of the species.

REYNOLDS: Does this mean spatial isolation, without any other factors operating, will produce a species?

WRIGHT: No, you've got differentiation, that's the first step.

SIMPSON: It's a necessary, but not a sufficient, requirement.

REYNOLDS: The word "initiating" is the one that was disturbing me.

MAYR: Speciation is normally a two-step phenomenon. Only in polyploidy is the isolation initiated as well as made irreversible in a single step. In all other situations the first step is the initiation of speciation by spatial isolation of populations, while the second step, the completion of speciation, the making irreversible of the isolation, consists of the acquisition of isolating mechanisms.

WRIGHT: And, of course, it doesn't give rise to new species unless there are factors causing these two isolated things to become different. They've got to drift apart—I mean, by selection or some other means. Selection is the most important factor. Condition of the selection is different in the two localities, inevitably.

MAYR: In different terms, you can say that a gene pool cannot be broken into two by internal events, with the exception of polyploidy. That's another way of putting it.

GODDARD: I like this last way of doing it, because it's a more operational definition.

MAYR: Yes, but most people wouldn't understand it.

BRINK: To get back to this question about projection, No. 4 sounds to me as though it were really set up for this because if you really mean that you have a stochastic process that can be described, then certainly in the next unit of time, whatever unit of time you use, you can make some kind of a probability statement about what's going on. That is certainly prediction.

WRIGHT: You can in a laboratory experiment make predictions on the basis of the

kind of stochastic process that does occur here, and it works.

SIMPSON: There are some specific examples that could be brought in.

BRINK: All right then, this does have predictive value.

WRIGHT: But that's not evolution in nature, it's just a laboratory experiment dealing with selection and the stochastic process of sampling.

BRINK: You mean that number 4 is only for laboratory work?

WRIGHT: No, I said you can only make a test of it in the laboratory. There is every reason to believe that the same thing occurs in nature, but the conditions are so complex that you could never get them down fully on paper and therefore can't make a prediction.

It's the same as in meteorology. There is nothing very mysterious, of course, about forces in meteorology but you can't predict weather accurately.

BRINK: I want to continue that thought then. If we grant that even under some limited situations, it has a predictive value with reference to setting up certain initial conditions and observing a result, then the last phase sort of knocked it right out. "Occasional unique events" takes care of every observation.

WRIGHT: You just have to recognize that they occur. Polyploidy and hybridization ordinarily don't occur, but once in a while do. There probably are groups that have arisen from a rare hybridization. Well, you can't do anything but say that.

MAYR: I agree with Brink that to include "occasional unique events" has caused me a good deal of worry, too. Perhaps the occasional unique event should be precisely spelled out, just as we are doing it now, in order not to permit inclusion there of all sorts of other things we believe not to occur. I think you have a good point.

WEISS: I don't see why, because it is only a definition of what you call occasional. If you have stochastic processes you've got a lot of occasional things happening, otherwise they wouldn't be stochastic.

BRINK: Then you'd cross it out if you include it in the stochastic.

WRIGHT: There are three elements here; (1) those that are directed and thus permit a determinative prediction based on a definite equation to which there is only one answer; (2) process in which you can define the variance but not the direction of change, permitting certain statistical predictions; and (3) unique events that you can't do anything with, either from a determinative or statistical standpoint, events that happen only once or twice in the history of the species, say in a hundred thousand years. Something that happens in one in a million germ cells, and thus in hundreds of cases in every generation in a species as large as the human, can be dealt with statistically. We would have to draw an arbitrary line then between these and things that are so rare you can't deal with them statistically in a reasonable way. It would be nice to leave this third category out but you must realize that they are there, and of great interest.

GERARD: The point is clear by now. What other significant comments are there?

HOTCHKISS: In the most beautiful sentence of all, No. 5, there are three terms, direction, adaptive and selection, which people argue about; I think we all know what is meant but adaptive, as used here, seems to me much more like a purposive kind of word than adaptive in some of the other statements. Especially in terms of populations where a change may only be adaptive on a short-range scale, that is, adaptive for the moment and then later detrimental from the over-all point of view.

I think something should be done to make this statement perhaps less beautiful but actually more realistic. One might say something like "direction, in the immediate sense, can be viewed as adaptive," or "would be in a context which is selective," or something like this. The idea that a direction can be adaptive is a very disturbing thing if you cut out some of the other words.

WEISS: I think you have a point because Miller, if I understood correctly, thought of adaptive in this context as meaning active adaptation, rather than a state of adaptedness arrived at by whatever means. This I think is what is meant here, isn't it? It

means the direction is determined by the result of being adapted? Not goal-seeking, actually self-adapting, but merely ending up, by coincidence or whatever you care to call it, as being fit to survive.

MAYR: The idea is still maintained, for instance, in continental Europe, that whenever you have directional trends in evolution they are due to "internal" something or other. It is important to emphasize that adaptation can be effected by natural selection; the exact wording I admit can be improved.

WEISS: The term "adaptive" is very ambiguous.

MAYR: One can almost leave the word adaptive out and say directional trends in the history of evolution are caused by selection, or controlled by natural selection, or something like that.

SIMPSON: I think we are getting back to the point that Miller already brought up that these succinct statements omit definitions. This sentence leaves three undefined words because we felt that in this group everybody knew what they meant. But if this is for public consumption those words should be quite precisely and extensively defined. To do this you have to give a little of the historical background, why one says this.

HOTCHKISS: Adaptive, well defined, would still not be defined so as to say adaptive in the short-range and detrimental in the long range. We need somehow this kind of thought; to include somehow those responses to immediate pressures which are not adaptive from the over-all point of view.

SIMPSON: It becomes tremendously complicated, even more so than your remarks indicated; a very complicated matter which has been discussed at great length.

HOTCHKISS: We could take as a simple example bacteria which have come to dispense with a certain growth requirement; if penicillin is present it will be able to kill these "adapted" bacteria when the growth factor is not there. So the change will not necessarily be for the better. And the reason we understand situations like this is because it happens with bacteria in one night. With populations in higher animals you not only go home to bed but you may wait several months or ages to make another observation, and so, for example, you don't see them for one hour adapting to use glucose faster, a couple of hours later having made so much acid that they are all dead. This for me makes of adaptation a sort of wishful, intuitive, much too purposeful word to be a general term.

MAYR: I thought we had discussed previously the question of how to define the term "adaptive" in a non-bacteriological sense.

GERARD: Are there other points somebody is eager to make?

WRIGHT: The thought just occurred to me that we are now using stochastic in an exceedingly different sense than you used it the other day, and there are these two uses, "working from an hypothesis" and "random." We could use random, random process, instead of stochastic. The two uses of the words have nothing in common.

MAYR: It might be better to use "random" in any case, because many people don't know what stochastic means.

GERARD: Our discussion has brought out a number of points in detail and a few in general. Our intent is to limit the statements to those on which respectable biologists probably would be unanimous; material that would be useful in presenting to students, or others, a map of the existing terrain. Despite criticisms in detail, this job has been done satisfactorily.

Several want something more, however, and I suspect that the reason one of the groups had great trouble in reaching a series of statements was that they were trying to achieve, in addition, the other objective, of integrating across levels. I feel this report has concerned itself with half the levels; the group has in effect said, "Here is a clear statement about this half, we are omitting the other half." Maybe this will come in under "Order," as Hotchkiss said; but if we are really looking for statements which apply to biology as a whole—let alone those which are valid above and below biology—and hope to get molecular biologists and population biologists thinking in comparable terms, at least recognizing what is common

and what unique about their problems, half of the levels cannot deliberately be eschewed. I point this out because, if I'm not mistaken, the other reports will not come out nearly as clean as this one, just because they have attempted to cover the entire level range.

Let us turn next to the functional group which had the greatest trouble; I don't know if they even have a definite report.

[The Chairman invited Dr. Brink to report for the "function" group.]

BRINK: Well, we didn't reach any common agreement at all as far as I can see, and I think probably you just now put your finger on the trouble. We were trying to see the problem all the way up and down the hierarchical line and had written along the side of the page in Gerard's paper where the items (boxes) are, all the units—molecule, organelle, cell, individual, group, species, community, biota. We were trying to see how we could look at it all in terms of the words and ideas represented in the columns. And one thing that did seem clear was that if you did use the general systems analysis—let's start at the individual and go back toward the molecule—one could first of all look at the individual as a total system in relationship to its total environment. All the essential connections are arrows which imply something about this stability or self-maintenance. Each one could be labeled. It would be labeled in terms of what was transported in or out, and we tried to decide what these things might be and we came up with the notion that it would be the familiar physical and chemical things like heat, radiation, and molecules and so on, which we'd like to lump under energy and entropy, going in or out of the system. And then there is a thing which we have no word for except this idea of information, some kind of contact which influences the course of events in adjusting to the outer environment. Then if you go to the organ, you have organ physiology, all the information about interrelationships between one organ and the rest of the organism, and you characterize the connections as indicated here, hormones, reflexes, etc. Drives would be in the nebulous area of information, which I don't know how to characterize further in the physical sense. The same idea obviously works for the cell; it has relationships to the rest of the organisms, to the organ, to the individual and, through that, to the environment.

Well, just pursue this right on down the hierarchy. Take mitochondria as an example of organelles; you get the notion that one thing mitochondria might contribute to the cell is ATP as a result of oxidative reactions. You could specify a good bit of the process. At this stage, we lost the notion of information, and in its place, we were using the idea of enzymes and catalysis which, it occurred to me this morning, was very well contained in Schmitt's statement, yesterday, that a cell is capable of ordered coupling of energy for specific synthesis. That word order implies there is something about information or design that isn't given a physical designation, like radiation or heat. So one could trace this same notion of information as being important to the organization of the net process that produces the stability, down to the molecular level. There one could isolate an enzyme system and draw a boundary around it, and have substrate going in and products coming out. If the enzyme weren't there in this system, these reactants would remain stable and wouldn't react. So the enzyme is the ordering feature of this system and the best way we know how to talk about that at the moment is something about specificity of structure, which is again coding of some kind.

So this idea does go all the way down the line. I didn't try to go on up toward the community but I think you see that it would work. There we would stop talking about catalysts and enzymes, and start talking about communications: actual words, radar, telephone conversations, and so on. This is the best we can come up with as a way to tie together the whole thing. It could be encompassed in systems analysis as generally understood.

GERARD: Are there any subsidiary comments from the "function" group?

HOTCHKISS: I might just underline a point or two. It seems that, if any con-

structive thought comes out, it was that there were kinds of transfer of matter, which could be more or less visualized—its actual movement, transfer of its energy content, or its actual conversion from one to another form of movement of its parts. Then there is this other class, the communications, the trigger mechanisms, the codes, a kind of unpredictably effective combination of structures. Maybe the very smallest level, the coenzyme, is the first of these and then next the hormones, enzymes, and so on.

These things have a disproportionate effect and it doesn't seem, to our present way of looking physically at them, immediately obvious why this kind of effectiveness is resident in this kind of structure. Of course, it goes up to the higher levels, emotion, patriotism, and so on. These set in motion things much larger than themselves and you can't with ordinary physical concepts explain how they have this implicit capacity. As implied earlier, we don't usually have observer entities sensitive to the appropriate aspects of structure.

There was one other thought. The kind of difficulty we had, very often, in trying to relate these to the hierarchical order was that the environment was more or less common to all levels. There was a tendency to relate the organelles to the cell, the cell to the organ and so on upward, but each of these is also in contact to varying degree with the inanimate environment. In the functional or metabolic aspect, perhaps more than in history and structure, you are obliged somehow to take into effect this double relation; relation to the thing below, the thing above; also relation, in a more or less degree, with a somewhat more unordered environment.

Various forms at the organelle level, like the nucleus or the mitochondria, seem dramatic enough to deserve attention; they are separated from the cell walls by a rather watery fluid which doesn't seem quite as interesting to some people. This may be viewed as another organelle, or merely an insulating barrier, but outside the cells there are ionic and other environments so that the cell is not only in contact with its own organelles and with the cells of other tissues, but with other cells of like kind. This is another thing we must not forget. So there are four expressions of the surroundings of each of these units, things of the same rank, those of higher and lower rank, and the inanimate environment. We were unable to make order out of this without rather complex flow charts and our tables weren't n dimensional enough.

BRINK: I just wanted to add one thing. The thought came up several times, a thing Weiss has mentioned often, the facts don't seem to have much biological meaning except when considered in relationship to the total system. You must always think of them in terms of the relationship to the rest of the system, higher or lower.

We say, "Yes, that's a well-described enzyme system, but does it do anything for the cell? Does it work in the cell?" We always have to come back to that kind of a question.

REYNOLDS: And the system includes, at a level lower than the organisms, the non-biological environment. We started out with the idea that we didn't want to have to consider the external non-biological environment until we get to organisms, and we found that that broke down.

MILLER: It may be desirable to separate some of the concepts which we have mentioned into subject, verb, object, predicate, in order to allow for the various logical relationships in these different aspects of flux. Being a process, it has these logical relationships, throughout both time and space, in a way that a completed act does not and unlike a completed structure in space which has relationships only to space and not in the other dimensions.

HOTCHKISS: Some of us have said that metabolism (i.e., function) is the same thing as the time rate of change of structure. This history thing is more complicated; it includes time changes of those average functional states of systems. We can look at the functional matters in two parts: there are the more obvious transformations of matter, and then there's this rather difficult to define triggering part where we infer the presence of properties which we have no way of measuring, that bear and transfer

information. The former builds up structure, the latter accumulates as history, of the organism.

Viewed in this way, the whole category of structure would be the single time-cross-section of the material transformations and the whole side of history is perhaps contained in single time-cross-sections of this conversion of the information element.

WEISS: Very good.

GERARD: There is a great deal of material in this for discussion; I shall take the privilege of pointing up some of it. The group has said essentially that the problem of regulation or equilibrium, or whatever term you prefer, involves the interaction of entities in relation to their subordinate entities, to their coordinate entities at the same level, and to the superordinate entity of which they are a part, and to the wider environment in which the large entity exists. Such a division into four types of reaction can be useful for exploring up and down the level hierarchy. Certain kinds of influence can operate between the entities, and constitute the means of interaction. Matter and energy are obvious ones, but the emphasis shifts from one level to another. This is reminiscent of what Mayr said yesterday concerning the changing importance of history versus structure; but, whether or not that holds, the transmission of what we are calling information involves rather different mechanisms at the different levels. This is a useful insight and suggests that different levels may well be characterized by the specific mechanisms of interaction. For example, to pick up another statement from yesterday, the forces between two entities in the physical world vary with the "strength" of the attracting or repelling bodies and with the distance between them. But if it is a Van de Waal force, distance enters as the sixth power; if a coulombic force, as the second power. It is useful both to look at them as a force varying as a function of distance and also as two different kinds of forces, with distinctive distance functions. What are some further reactions to the presentation?

GODDARD: Well, I could make one comment, Gerard, which I'm afraid is perhaps obvious to this audience but has only been obvious to me for the last few months. I've long been aware of the fact that the genes transmitted information to the organisms but I had been struggling with what Brink has talked about, that a great deal of information in the cells is obviously built into the molecules—not only the gene molecule, but molecule of enzyme, of coenzyme, mitochondrial structures, and so on—and that we have a dual transmission of information from one generation to another. We have the information which can be handled very nicely by the geneticists because it occasionally varies, which gives you something to work with in terms of mutation, chromosome arrangement. And, secondly, because it doesn't arise *de novo*.

You have a different sort of information which is cytoplasmic and may be, therefore, more irregular in its transmission, like the transmission of plastids or mitochondria.

You have a third sort of information which has been largely ignored by the geneticist but is in a common current with the physiologist-biochemist, who had mistakenly thought he was working in a different area from the geneticist, and that is the information carried on by such simple molecules as glucose 1,6-diphosphate or fructose 1,6-diphosphate. Information is carried by these substrate molecules as well as in the enzymes, coenzymes, and mitochondrial systems and in the chromosomes.

The mechanisms of transmission may be different. In some cases the cell is left with enough structure, enough information, in the rest of the system to reconstruct the missing component. I suspect very much that there are other cases, perhaps the myosin molecule, which can't be reconstructed if there isn't a myosin molecule to begin with. Maybe the genes transmit enough information so that the first myosin molecule can be built up, I don't know; this is a matter of experimental determination. But because I am so new at this business of information, I have been extremely interested in what Brink and his group have been able to say, thinking of it now at the

cell level and realizing that the interactions have gone all the way up the line.

GERARD: Any other comments? If not, I will throw in another aspect of regulation, not explicitly mentioned, which also involves all levels of the material—the aspect of boundaries, or tolerance, of the system. How much information is necessary to keep it acting? How far can it be distorted and still restore itself? The whole area of equilibrium and stress and tolerance limits—when displacements remain reversible and when they become irreversible—deserves attention. Then come the questions of health and disease, of organic and functional. All such considerations flow from this kind of approach. The additional question being asked about all these systems (besides "What are the normal entities that are interacting and what are the mechanisms of interactions?") is, "How far can it be disturbed and what happens after the disturbance?"

BRINK: This is the experimental approach to the problem, isn't it? It's what you always do. You disturb the system and see how it comes back, or whether it runs off and goes into some other form which is no longer living.

GERARD: That's right. Essentially just that question is at issue when comparing function and structure. When a functional change has become irreversible, structure results. Any other comments? If not, we are ready for the "structure" group.

[The Chairman invited Dr. Weiss to report the findings of the "structure" group.]

WEISS: We have no written statements. We don't even have, perhaps, anything I can present as the agreement of the group except as I interpret it, and for that reason I'd like them to take exception any time I say something they think they cannot share.

One thing quite generally felt, and it has been expressed here before, was that dissecting structure from history or from function is an artificial dissection. But so is any type of a scientific classification. Structure or form, as pattern in space; function, as the patterned process; and history, as the change in time of the pattern of the processes; are obviously three aspects of a single continuum. Structure is perhaps comparable to a cross section, history comparable to a longitudinal or frontal section through that continuum, the time line being the antero-posterior axis, if you want. And so, if we understand this, the relation is quite clear.

However, it has some intrinsic difficulties, and that is that the dissection cannot really be carried out cleanly. And I am glad you reversed the order of these items on the list—that's the logical order—because structure is inconceivable except in terms of structuring processes. If structure is merely a sample or cross section, arresting for a moment processes that proceed in time, then of course the basic thing in all this is the underlying ordering processes, rather than the resultant indicator of those processes, the structural order. If that's understood, then we can engage in this artificial task of separating structure from function, provided we are conscious of the fact that there is no sense to the question what comes first—function or structure. These are the two sides of a coin, in a way, but since we don't always look at both sides of a coin at the same time, it is perfectly all right to look at structure at one time and at function another time.

Then we went through this [material in Gerard's paper] and found quite clearly that there are levels here characterized by the fact that a certain relative constancy of the phenomena is observed at the higher level without corresponding constancy at the lower level, "higher" and "lower" in this connection referring to a hierarchical order of systems in which the lower one is always enclosed in the higher one, like a set of Chinese boxes.

As we went up the scale, there was nothing really to discuss except a few eliminations and additions of samples. One: molecular level. Two: orderly arrays of molecules. Three: assembly of such orderly arrays into an organism of relative constancy—the cell. Four: an array of cells into tissues, organs, and systems. Five: the array of those units—the organism. Each of the lower ones being actually physically, materially, enclosed in

the next higher one. Going from the individual organism up to higher-order collectives, a little trouble arose, which was essentially logical, and which we straightened out with your help.

In my personal opinion, there was no clear-cut distinction on any of these levels between the establishment of the existence of sheer heterogeneity of the units of a level and the particular order in which this heterogeneity persistency recurs. This distinction is basic. In contrast to random sortings, biological systems also display sortings of definite order, characterized by seriation in time and definite coordinates in space. We are thus faced with different degrees of order, which have not been spelled out. We didn't do anything about this, but I think it's important to point out that there is a fundamental difference between just plain heterogeneity and ordered heterogeneity; there are assortments, and there are ordered assortments, where the *order* of the assortment is the feature that counts.

Several concepts, like specificity, for instance, run through all the groups—as a matter of fact, you have it in 1 and 5—and therefore don't have to be assigned to any one in particular.

As we get up to the highest group, there we face this first logical dilemma of classification. If you classify the higher collective of individuals by criteria of derivation or origin, then you arrive at a super group of kinship through the family, or the tribe, or the species. If, on the other hand, you arrange the higher group of the collective according to the functional interrelations or living conditions of its members, you arrive at such units as the biotum or the ecological niche, and so on. In both of these classifications, you are dealing with groups which are on the same level, consisting of the same number of units of the same character, but the units are grouped according to different principles; one is a natural, ecological principle and the other is a logical principle, which you have to reconstruct from studying the history, the derivation. So these grouping principles are not inherent in the natural systems, but they vary within the system you adopt in ordering the data. So there is a dichotomy here.

Then going up to 8 and 9, we did essentially what the Wright, Mayr, etc., group did, and disqualified ourselves from making any evaluation, because we felt we were not competent to establish that these are two super-ordinated sets of systems.

Essentially our evaluation has been that, by and large, it's a very workable scheme. There have been minor disagreements as to whether this or that example should be incorporated.

GERARD: This might be a good occasion to remind the participants from the Biology Council, and to tell the rest, what this document really is—a first draft of a statement to the Council. When the Council looked at it, and it became obvious that the material was too complex for quick handling, the suggestion was made to hold a conference lasting some days, and really work with the document in detail. It is partly for that reason that I have been a bit insistent that we actually look at some of the detailed tables.

WEISS: Well, this is essentially the spirit in which we have taken it. There is one thing, sooner or later, which I would like to add as my personal comment, as to the cogency of talking of levels. There are certain limitations to when we should use the term "levels," which we have never defined here, and if this is the time I should like to say a few words on that point.

[The Chairman interrupted with the suggestion that other members of the "structure" group make such comments as they wished on Dr. Weiss's report.]

GODDARD: I would only say, and this is an individual reaction, I think it is quite possible in this area to do exactly what Mayr, Simpson, and the others have done in their area. Some of these statements seem so primitive that one may be reluctant to make them, but they are not primitive if you really begin to examine the content and the meaning. I did, with this model in front of me, make such statements for myself this morning. I am not going to present them to the group now—they are not any-

thing that has been criticized and evaluated—yet I have a feeling that there probably are something like fifteen or twenty concrete statements that could be made about structure which might seem at first a kind of pious restatement of what we have learned in biology but, if put in operational language, have very real meaning.

WEISS: Well, I think the statement, for instance, that form, any form we deal with, is the result of a formative process, is a very important statement. The real thing is the order of the processes leading up there.

WRIGHT: On this business of dealing separately with these columns: form, history, structure, process, etc., my own use of this two-dimensional scheme—hierarchy and just those columns—has precisely the opposite purpose to that of separating these things. It was to show how I propose to cross boundaries from one field to another.

GERARD: You can't cross a boundary until the boundary is there to cross.

WEISS: This brings out a certain danger, which has nothing to do with our assignment on structure, but is more general. To the question of how far one should go with this type of dissection, I don't know the answer. But to what extent does this attempt at a clear-cut dissection simply promote the present trend to fragmentation unless it were counteracted by efforts to reassemble the pieces?

GERARD: The discussion at the moment is in terms of what we will do with our product, which we agreed to postpone. Caspari?

CASPARI: I don't have very much more to say except to re-emphasize that there is in this hierarchic order which we have here a very decided break between items 5 and 6. From 1 to 5 there is really not very much discussion possible; this is something that is quite self-evident and which everybody accepts. The relation of 5 to 6, and 6 to 7, apparently is of a different kind. There is not only dichotomy but also this question of what we look at as a primary group, as a population; the same thing apparently is going on in 8. It seems to me this is where the problem really arises, this break between 5 and 6. And that goes back to the question of what an individual really is and the boundary, as exemplified by membranes, in No. 2.

WEISS: That is part of what I wanted to elaborate.

MAYR: It is an interesting coincidence that this was the most severe break that our group felt. Many people have expressed in the past that when you deal with the individual and its components—or the zygote as the geneticists might say—and with populations and above, you are dealing to a considerable extent with two separate phases of biology. If this study should help to confirm that, as in a way it does, it doesn't mean that this is a complete break—there is an overlap and a connection—but it is of all the steps perhaps the most pronounced.

WEISS: I don't like to see the significance of this break over-emphasized, because there are breaks in other areas. The coordinated molecular group activities that result in the "symbiotic" molecular system that we call a cell, that is, the interdependence of molecular processes on the cell level, is something which you don't find in dealing with random dispersed molecular groups, mere populations of molecules, and that break is quite as important as the break on the next level at 5. Truly, the mechanisms that keep our biochemical systems interrelated and mutually supporting are different from the mechanisms that hold the atomic nucleus together.

GERARD: We are rediscovering that physical science, biological science, and social science have some legitimate reasons for being broken into these large categories.

WEISS: But they are formal relationships, and there are very great dissimilarities or even complete inconsistencies in the methods, or techniques.

GODDARD: I would like to comment that if I take the area of structure and try to formulate statements, one of the first things that happens is that I go right across into the area of the history, since if you start to discuss structure you begin to put in the information. That is, if you stop to talk about structure you have to include the chromosomes and the inheritance. Further,

if you start to talk about a structure you find that your statements are bordering on function, because you recognize that structure is reproduced by process; you cannot discuss structure aside from the development of the individual itself, as well as the evolutionary significance. Structure has had an evolution, a development, so that even though one dissects out the problem to look at, if you attempt to put it together with any knowledge of modern biology, you do not reconstruct a classical histology, because you are not content with what you want to know how it came. You cannot ignore genetics, or evolution, or embryology, or function.

WEISS: Well, there is one thing I would like to add to clarify our dealing with structure, because it's largely semantics—it's simply a question of what do we want to call structure. [Diagramming on board. See Fig. 2]. Suppose you have a horserace (*a*). Starting out from here, there are five horses, and this is the course of the horserace. Suppose this is the finish line, and at the finish line the field comes out like this—let's make it simple. The distribution of the horses at this point is essentially "terminal structure," which we can see manifestly exhibited here; but inherently it has already been present in the potential kinetics of the individual horses right at the start.

The second thing I want to stress is that the terminal structure will not always come out that way. For instance (*b*), it would come out differently if the track had been wet the day before but drying up from one side, so that there would be a biassed handicap. As a matter of fact, it may not come out at all like this because one of the horses may drop out altogether; or if one horse fell into the path of the next horse, then the next horse would be "inhibited." These accidents of structure are not determined by the genes, which somehow compare to the starting kinetic properties of our horses, but of course affect the eventual structural result. You see why I am interested in seeing that all participating processes be taken into account. I hope that biologists will get to the point that when they speak of structure and order they will think in these terms rather than in the static terms of conventional anatomy.

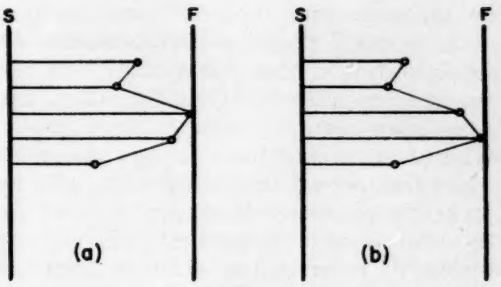

FIG. 2.

GERARD: Does anybody else want to comment on this figure? I want to, before we leave it.

SCHMITT: I want to comment eventually on the thing this figure was meant to do.

GERARD: First, if we chose a different time, the order would be entirely different, so the order depends on the criteria. This is obvious. Second, what is in those horses when they start includes more than genes.

SCHMITT: Apropos this business of morphology and function, 30 years or more ago to say you were a morphologist or physiologist implied important distinctions concerning thought habits and experimental approach. It seems to me that temperamental differences make one man say, "What is structure?" and treat this problem as in a descriptive, taxonomic sense, or "What is function?", and to describe function in more or less biophysical or biochemical sense. This is something which is inherent in the individual and in the caste system, of the union, to which he belongs.

It seems to me that the time has come when all of these things have to be unified at the level at which they occur, and perhaps the question asked, "How did it all get that way?" There are overlapping ellipses, as I have drawn them here, between biology, physics, chemistry and mathematics, and where they overlap we can all talk in common. This is one way of looking at it. Another way is to have a pyramid with the base a triangle, which you can label any way you like—physics, mathematics, and chemistry with an apex of biology. But, eventually, I think that we have to come to

an understanding that all our structural and functional problems resolve themselves as properties of the atoms which are primarily responsible for building them up. The carbon atom is certainly a very specific type of atom and most of our structures depend, in one way or another, on the specific properties of carbon. Hydrogen, in turn, has its own special properties and without hydrogen bonding, or without aliphatic chains, we wouldn't have two-dimensional, lipid lamellar systems like we do. Nor would we have protein molecules except for the special properties of nitrogen atoms. And so we can eventually understand the inherent basic properties of structure in terms of the atoms of which they are composed.

This is satisfying only to a certain extent. We have to get, then, to the bottom of the matter and be able to predict something. Then we will be operating in the middle zone, which is not really structural—it is not really functional—it is not really historical, but it has to have components of each in its thinking at all times. I think this is what we need somehow to convey.

MAYR: Do you think it would be useful to try to minimize the separation of structure and function?

SCHMITT: Well, nothing succeeds like success. If you can eventually show, in any given system, like muscle, how the macromolecular system interacts with the energy source in producing change in length or tension; or, if you could succeed in showing how in a nerve impulse the sodium ion combines with an enzymatic mechanism in order to change structure sufficiently to produce a change in permeability, you will have been successful.

MAYR: But you have it, of course, at other levels, and I give you an example. In a treatise on taxonomy I spoke of morphological characters, physiological characters, etc. No sooner had I written it down and started to discuss it than I said to myself, "This is nonsense, because all the morphological characters are just as much physiological characters as the ones I had listed as physiological." I still kept the headings, because this was something traditional in the field, but I stated as the very first sentence in the section on morphological characters—"These are not really any different from other physiological characters except we see them in the structures they produce." That is in a very different level from what you talked of, but it comes out essentially the same way.

Now I wonder if it isn't the same sort of thing even deeper down, in the field of physics, where they have the greatest difficulties distinguishing what is matter and what is energy, because there are equations that let you look at it one way or the other. Isn't this, perhaps, an outcome of that basic duality of everything?

SCHMITT: Yes. What is the significant structure in an enzyme molecule, which is globular, contains internal helices, but which at the end points, where the chains are warped, are not helices? And these are in the regions, say, where the prosthetic groups are. Well, where is structure, where is function?

CASPARI: I am wondering whether we are not beating a dead horse, because I don't know whether there is anybody who would describe structure without these points in mind. If anybody describes structure, he either describes it in relation to function, or he describes it in relation to history—in the developmental sense or in the evolutionary sense. The only problem, it would appear to me, consists in deciding on a way of getting all three viewpoints together. But this seems to be difficult, and therefore, the usual procedure is to describe it from one point only.

WEISS: No. Caspari, I think you either are too generous or your experience has been too limited in these regards. I think that plenty of people abuse these terms very badly and do not think in the way we are thinking now. The main point is to know what we are talking about when we talk about structure or when we talk about function. All of us here seem to know. But most people—particularly those who learn only from textbooks—don't know, and consider these two things as hard-and-fast separable and incompatible categories. When these

terms assume connotations that really nobody in coining such shorthand reference labels had put into them, they become hazards. Therefore, I believe in safeguards against the use of any such term other than merely as a label for a well-defined, well-known, and intercommunicable content.

GERARD: I would like to make an important point here. The structures which make possible a particular function, or the function which is the result of certain kinds of structures at any moment, are likely to be in different levels. That is, one must look at structures at the macromolecular or organelle level to see how a muscle functions at the cellular level. I think this is generally true up and down the levels and is important to keep in mind.

MILLER: Apropos the earlier statement of Caspari about the difference between levels 5 and 6, I would hope that any product that came out of these discussions would deal with problems like this, because even though to some people it might be apparent there is a continuity, I think it should be quite explicitly spelled out why we feel that there is a continuity between these levels.

The major difference between the social and the biological sciences is probably the greater concern in the social sciences for information transmission and in the natural, both biological and physical, sciences for energy transmission. Obviously, though, there are similarities between the biological and social phenomena. The main difference between the individual level and the group and higher levels is that the locomotion of the individual parts is independent—connections by the cell walls have been lost—consequently, there are differing distances among the components. Therefore, action at a distance becomes important, and just as it's more efficient to wire money so it's more efficient and, consequently, has higher survival value to send information which will result in action over these varying distances than to accomplish it by energy transmission. Thus, in the autonomic system, there is transmission of information over very small distances by neurohumoral agents across a synapse, but also action at a distance by secretion of adrenalin from the adrenal medulla, which then activates an effector organ some distance away. This is similar to the transmission of a letter, just as the sending of information on neurones is like the transmission of messages on a telegraph wire. Many of these phenomena are clearly comparable, but such generalities are not ordinarily included in our conceptualizations, because the components of superorganismic systems move independently in space. I would hope that discussion of principles like this might be included in any product.

HOTCHKISS: This reminds me that we discussed for a time Mayr's statement that with the lower levels of integration you are occupied more frequently with form and structure and at the higher levels with history. This is not to say that history and structure are not represented at both ends of the scale. As mentioned a while ago, structure and history can be looked upon as single cross-sections in time of the constantly occurring transformations of matter and energy on the one hand and information on the other. The statement Miller has just made is a very parallel statement; with the lower forms you can be occupied with the energy transfers and with the higher, more typically and more usefully, with the information transmitted. There are these dual aspects of the same thing—Miller is making Mayr's statement with the time variable left in. Both are speaking of what it is useful or usual to study, but I want to say again that the lower levels do have information transfer and therefore a history. It is observed all along the line, for example, it is more efficient for the pituitary to send hormones (information molecules) down to some tissue, than to send a whole lot of glycogen (end product) or something—this is the same principle, and I think it is a very good one.

This brings me to the other point—the apparent break between the individual and the group. It should be emphasized that this is not a very real break. I would even like to throw up the simple idea that it may result from a kind of anthropomorphic way of thinking. We can feel like an individual, and we can represent a cell as a free-living

cell and think of what it must be like to be one. As soon as the cell becomes part of a higher organism, we begin to think of ourselves as that higher organism. Always our simple way of vicariously experiencing this is as the individual. You might look upon development and differentiation as a movement along this hierarchical axis from a free-living gamete, which can be followed as an individual, through embryological development to an individual in which it takes the whole colony to have the net properties of an individual. So you are moving, depending on how you look at it, up and down the axis, and I don't think this is any different from what happens in acculturation. When a child comes into the world, it is a rather self-willed and self-determined sort of thing. Then, gradually it is accultured until it begins to perceive what things one can and cannot do to other individuals, and doing so it becomes part of the group or society, more predictable, less necessary to deal with as an individual.

WRIGHT: The importance of information at the higher levels, and relative unimportance at the lower levels, seems to me a little bit questionable. The only function of the genes is transmission of information, resulting in the impression of specific pattern on enzymes, and of antigenic specificity, which is very apparently more directly determined by information from genes than any other character we know. Antigenic specificity and enzyme specificity are certainly pretty fundamental things at the lower levels. These are not due to transmission of energy—something else supplies the energy there—it's the information that is the significant thing.

HOTCHKISS: Just one thing in rebuttal. I didn't feel that information is less important at the lower levels. We have customarily considered it less. That is just the point our group wanted to make, that we now see the obligation to deal progressively further down the scale with the same concept.

MILLER: May I rise to the same point, because I don't want to be misunderstood any more than Hotchkiss does? Traditionally, there has been much more emphasis in natural science on energy transmission, and traditionally in the social sciences on spoken, symbolic transmissions, which are information transmissions. Obviously from what I have said previously, I believe that energy and information are always transferred together, and that we need to make both quantitative statements concerning energy characteristics and quantitative-type statements concerning information characteristics. Nor do we need to argue as to whether one or the other is more important at one level or the other. My point is simply that biologists should pay more attention to information transmissions, as is being done in genetic studies, and that social science should pay more attention to energy transmission.

GERARD: Another good example of finding similarities which are useful and then perhaps, tending to neglect the differences—which remain also useful. There is a striking difference between the kind of information that comes in genes and the kind of information that goes along nerve fibers. One is more like a book, and the other is more like a spoken or telephoned message. The time element is enormously different, and must not be neglected. One shouldn't throw out a linear change too easily because, without going into the social at all but just looking at recognized biological organisms, the essential axis along which we judge an organism as "higher" and "lower" is the relative importance of transmission. It is the richness of the behavior which it permits that is great in the higher systems.

WRIGHT: In connection with the species as an entity, you've got a mechanism of transmission of information, although it may require tens of thousands of years for a mutation which arises, say, in the eastern United States, to gradually work its way by successive crosses to the Pacific. You've got an entity in the species which is literally continuous in space-time, because of the continual coming together and separating of protoplasms. You can look on it as a sort of myxomycete in space time which has literal, physical continuity. And yet it does bind it all together as one unit, so that what arises in one place ultimately has an effect elsewhere, and the whole thing evolves as a

unit unless some isolating principle comes about that separates it into two units.

GERARD: This is not in any way contrary to what I said.

WEISS: I would like to ask a completely innocent question. I hear about this information theory and the genes; it is coming up more and more. You refer to it in the simile of a book. What good is a book unless there is somebody who can read it? What good is that information unless you've got somebody to decode and decipher and translate it into his own words? In other words, how can you convey information unless there is a receptor reciprocal to that information? What is it in the case of the gene?

WRIGHT: It impresses a certain pattern.

MAYR: On the substrate.

WEISS: But you don't impress a book on anybody who doesn't have the vocabulary, who doesn't have the. . . .

WRIGHT: Well, Gerard will defend the book. Template is the usual figure that is used there.

WEISS: You mean impresses a pattern on plastic goo?

Wright: Yes, well, plastic goo may not be the best way to phrase it, but. . . .

WEISS: Well, I think this is a very basic difference of concepts, and I think this is one that deserves a whole conference. I just wanted to. . . .

[The Chairman now called on Dr. Weiss for a brief statement on "levels."]

WEISS: Well, I may be able to do it very quickly. My point was that the gist of all our discussions, if I see correctly, is as follows [illustrating on blackboard]: If this is a box (See Figure 3) containing units—now actually what I'm doing here is illegitimate, taking the units out of the box—this is the higher level (A) and this, of course, is composed of the units of the lower level (a), plus. The "plus" is not necessarily substance, but relations. Now, it seems to me that if you are dealing with independent units here at the lowest level (α) and you assume the normal variance of events, then by superimposing all the individual cases that could occur you would obtain the picture of a bunch of processes or physiologies or potential developments or whatever, for each one of those units as indicated by the divergent lines. Nature being as unpredictable in detail as it is, this would have to lead to a whole bunch of potential results. That is, if you followed over a long time all such near-identical units, you would come out with a bunch of terminal products showing a much wider spread than was present among the initial population. Now, the truth, however, is that if you pile up these individual events, they do not progressively diverge, but they lead to certain common states which have sufficient identity to be recognizably similar; that's why we call these states "individuals." I think this is the test of individuality. Therefore, you must assume that there exist certain actions of the group, when they are in a group, which restrain this complete freedom of excursion and which constrain or restrict the number of degrees of the constituent elements in such a way that their collective product will have a much higher degree of *invariance* than does each individual member of the group. This is the most general expression of biological integration I can give you. There must be principles which tie these bunches, so to speak, together, so that they won't indefinitely diverge, but will converge. And all of what we have said about equifinality and so on is really subsumed in this general statement. Now, if these were cells, you can actually demonstrate experimentally, with the technique of

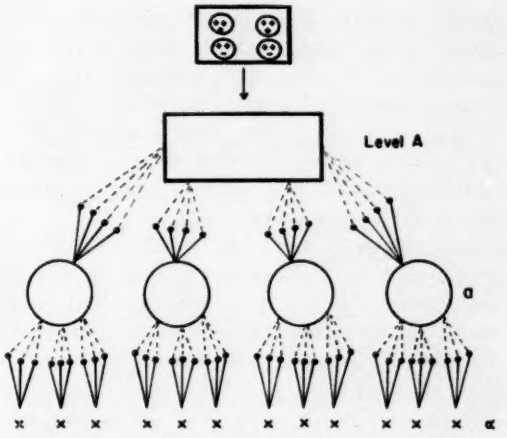

FIG. 3.

tissue culture, that they can undergo a great many more changes than they normally do. The fact that normally they do form a liver or a kidney, etc., must therefore be due again to certain directively restraining actions exerted by the collective on the freedom of variance, so to speak, of these boxes of the next lower level.

My point is that the whole system of levels and of individuality, and so on, is contingent on the fact that we find such nodal points where things have a relative degree of stability and invariance, or at least enough durability and stability to be worthy of a name. And that's what we call an individual. If it weren't for this principle, there would be no intermediate stereotypes that we could recognize as typical organs or cell types or cell organelles—or if we want to project upward, firm groups of individuals in society. Now, there is a very important lesson in this, I think, and that is that we should reserve the term "levels" to the relation between two such systems where this type of control, whether you call it regulation or coordination or what not, exists. At the same time, if we recognize this and acknowledge it, then it hides the very profound biological problem as to what these things consist of. Because the liver does not then deal directly with the molecules of a liver, but through the agency of an intermediate control step, which is the cell. The organism or the society does not work with molecules but through a hierarchy of intermediate steps, so to speak, which bunch the component processes of lower orders together. And my suggestion would be that we be very strict in using the term levels, individuality, etc., with a concept of this kind in mind, and study what the real content of this type of a pure symbolic formal picture may be.

GERARD: This is an excellent statement and puts in place a number of things that have been said or implied. It recalls Whitehead's original statement as to what constitutes a system, or an organism in his sense; something like, "An electron runs, but in an atom it doesn't blindly run." A restriction is always involved where a subordinate system is put into the superordinate one; that is what makes it a system. That a meaningful system must have adequate endurance is also well emphasized. I would tie this up also to my comment a few moments ago, that when you look at the mechanism of action at one level, you always are forced to look into structure, to consider elements at the subordinate level.

WRIGHT: Each level is defined by a particular integrating principle. At the level of the species, the integrating principle is biparental reproduction. But at the same level you have a wholly different integrating principle, i.e., social instincts, which leads to two entities that cut across each other at right angles. Just as in the human society an individual can belong to both a church and to a profession, belonging to one integrated entity doesn't prevent belonging to another. An aphid or an ant can belong to a certain species but also to a certain social system which includes a lot of other things that have no genetic relation. In the relation between the species and biota, the world biota consists, you might say, of species. But biota of any particular region is composed of species connected by kinship with populations, perhaps all the way across the continent.

REYNOLDS: Something I think ought to be underlined—you have mentioned a couple of times that function derives from structure, and there has been some question as to whether or not this was recognized. I have the feeling that fact that structures have functions is pretty well recognized. The thing that Weiss brought out that I thought unique and stimulating was that structure derives from function. In other words, a structure is a cross-section through a process. The other is also true, but I think this is the new aspect.

MAYR: My two points are: (1) where you have no boxing you don't have levels. (2) The only thing we can do, as a first step, when we speak of one level and want to analyze and explain it, is to do this in terms of the next lower level. The attempts that some have made to, let's say, explain the courtship of a bird of paradise in terms of molecules, is an utterly absurd one. This is the sort of thing that chemist and physicists

sometimes attempt to do or suggest be done, not realizing that we have these distinct levels.

GERARD: We must shortly break. I shall call on Brink, and then on myself, then on Wright, and then we will close.

BRINK: Well, I would say that this kind of reasoning would be one of the best ways to tie physics, chemistry, and biology together, because I think this is implicit in all physical and chemical reasoning—the recognition of intermediate configurations of smaller items which have in themselves certain relationships to other larger aggregates.

GERARD: My point also exemplifies how one can get belated insights from this approach. All my professional life I have been a little unhappy about the levels between cell and organism—tissue, organ, organ system. Organ and tissue seem too much alike, and the relation is not an encompassing one—not all muscle cells are in one muscle. But both are valid: the genetic relationship gives the tissue and the ecological and functional interaction or relationship gives the organ.

WRIGHT: The point I was going to speak about was the bird of paradise and molecule. I'll agree with the absurdity of trying to explain the question of behavior of the bird of paradise by molecules. In the case of evolution we have the species, which is a very loosely-knit sort of an organism existing over a tremendous range in time and space with very slow integration. At the opposite extreme we have the gene which is a type of macromolecule, separated by several levels, cell, organ, individual, from the species—you can make as many as you please here. Yet for certain purposes, not all, we can deal with the mechanism of the evolution of species, as a unit in evolution, as an assemblage of gene frequencies. That is, we skip all the way back from the species to the gene, because of the mode of reproduction. The individuals don't reproduce individuals, as such, in most cases. The genes are assembled from the entire species, so that you have to pass over this long series of levels to get at the simplest way of describing a species for certain purposes.

[Break for lunch.]

GERARD: What shall we do with all this? I would like to tie it in with the discussion toward the end of the morning; because whether using a hierarchical, analogical, or other concept, much the same sort of things can be said about institutions and bodies of knowledge as about organisms and their bodies. What emerges is a structure. As a minimum, we will put down certain sentences concerning relationships and they will be a body of knowledge or doctrine. What has been really worthwhile, however, has been the process we have gone through in forming this body; i.e., the functional-historical interactions which led to these conclusions. Alas, it is always the fixed structure that gets perpetuated and institutionalized. A process of discovery is progressive in time, but it crystallizes now or then into certain departments or institutes or other formal organizations; and after this happens it takes something like a molting to get them changed. So, as an outcome of this meeting, our objective should be, as much as possible, not to present a finished or a temporarily set body of doctrine, but to transmit a process.

This is especially relevant to the teaching problem. Passing this effort on to students as a living experience almost necessarily means passing it on to teachers; for the other way of reaching them, through documents, is too static. One idea that has been bouncing around the Biology Council for a long time is to have "research clinics," groups of two or three distinguished biologists who would visit the smaller or less favored institutions and discuss research in plan or progress. We might consider teaching clinics of the same kind. Members of a group such as this might lead groups of biology teachers through experiences comparable to those we have just had. An edited tape recording of sections of our discussion might also be use-

ful to start others on activity thinking about biology. Paulson, will you say a word now?

PAULSON: There are a considerable number of regional, state, local conferences held each year under varied auspices, at which teachers do try to grapple with this kind of problem, both at the college and the high school levels. They would appreciate some kind of guidance from those who are most informed as to what the relationships in biology and between biology and other disciplines really are. At a conference held under the auspices of the NABT in Gainesville a year ago, high school and college teachers and administrators attempted to find ways in which each group could do things to help the teacher do a better job, and help the students learn more effectively. Each of these speakers, knowing the interrelation of his own field with numerous others, had a focus which ranged out pretty readily into the whole of biology, and the total listing was a pretty encyclopedic one.

When people came to combine these it was pretty clearly apparent that there could be found some kind of common framework which would be more illuminating than just this serial listing of the contents of separate fields. Time, and inclination, perhaps, forbade their actually doing this.

The example of this type of conference, together with the type of conclusions that it comes to, not necessarily the specifically formulated set of concepts, but the processes by which they are arrived at, would be a most useful model. The same kind of thing goes on, of course, within college departments and elsewhere. Another way of reaching teachers, college people, or any other group, is through some specially constructed summer course experience, where one attempts to bring them closer to the frontier. Again, the kind of ordering which has been explored here might be an excellent framework for such a presentation. There are all kinds of other possible modifications.

I think it would be extremely helpful in a variety of ways for information about deliberations such as this to be disseminated not merely in a learned journal which would be read by the more alert and curious, but also through, say, the science teaching journals or through some publications which might reach a large number of college teachers.

WEISS: I was just thinking how to make these things come down to earth. Would a group like this or a related group be willing—if the Biology Council or some other organization put at the disposal of each of them a selection of twenty of the current textbooks in biology—to go over the concepts as there stated, and take issue with them? Instead of starting from scratch as we've done here, start from what there is and see whether it needs amendment or whether we take exception. We'd find out what the state is—maybe it isn't so bad. But I believe it would be a very, very revealing thing and this is one possibility.

GERARD: The Physiology Survey examined a number of textbooks of biology, not as dynamically as this because perceptiveness on a large scale is hard to obtain, and the impression was pretty bad.

WEISS: Of course I wasn't thinking of making a systematic survey; simply glancing it over and stubbing your toe on some of the most offensive conceptual statements, particularly the ones that vitiate our research and teaching.

GODDARD: I was going to say, Weiss, I think one might also find some concepts pretty generally recognized running through a biology book.

WEISS: Well, but that's included in my proposal. Simply look over and see which ones we agree with and to which ones we take exception. But if we do take exception, spell out why.

MILLER: That might be profitable so long as we don't have to put on the Good Housekeeping Seal of Approval after it is done, but rather do it as a group of individuals.

WEISS: This is not to become any indoctrination; the results would be essentially for our own guidance.

GERARD: I have no objection to the group doing this if it is willing to take the time. Perhaps it should be referred to the Education Committee. What worries me about going back to the textbooks is that I strongly suspect one will find the right words in the right places, but they will simply be exhib-

ited like the fare in a cafeteria. One of the most popular elementary texts in general biology is absolutely nothing but a dictionary—"organisms are made of cells, cells have cytoplasm and nuclei, nuclei have nucleoli and chromatin"—a dreary procession of factual statements. There was nothing to take positive exception to, but the volume is utterly and completely dead and useless.

WEISS: This is another question, and the Educational Policy Committee is dealing with this. But as for passing on the concepts as they are being used, this you cannot delegate to any one committee. This group has been assembled for that particular purpose.

GERARD: Paulson, have you a comment?

PAULSON: Dr. Schmitt has repeatedly referred to the utility of this kind of thing in defining what the challenges are, where the frontier lies. That would be most useful to the average college or high school teacher. Not being familiar with more than a small segment of biology, he may not really know where the frontiers are, although he suspects that there are some.

Most textbooks have a perfectly dreadful finality about them and this isn't the kind of thing that makes students want to go very far. The students themselves, given half a chance, will start asking questions which may lead you to a frontier, although it may be the frontier of your own ignorance rather than a frontier of biology. If a presentation of a conceptual framework could be coupled with some indication of what are the unanswered questions, that might be most enlivening to the whole biological education process.

Finally, the relation to other fields, social sciences for instance, as was pointed out could bring biological education into a much more realistic relationship to other departments of instruction than it now very generally is. One would perhaps be moving toward what general education policy *says* it would like to do and away from what it too often finds itself doing.

SCHMITT: There was an annual started, of which two volumes were issued, called *Survey of Biological Progress*, which was meant to do something like that.

WEISS: It's still going.

GERARD: As has been said, a number of groups have had meetings of this kind that were very good. I was mixed up with one, held by the National Science Foundation in Oklahoma a couple of years ago, which was enormously successful from the point of view of the college teachers who attended.

One suggestion from Weiss's statement—we might see if valid principles have been overlooked or false principles promulgated. Is this your interest in looking into textbooks?

WEISS: No, just so we would have something more solid to wrap our statements around; concrete examples of the way "concepts" are being used, how they are being applied, how they are being defined, what people do with them. You can have it both in research and in textbooks; those are our best sources of information.

[In the brief time remaining to the Conference several suggestions were made for extending and enlarging the effort.

Drs. Miller, Reynolds, and Weiss suggested the compilation of a selected bibliography of books and articles dealing particularly effectively with conceptualization in biology. One way to accomplish this would be to have each participant submit a limited number of titles deemed particularly worthwhile.

Dr. Miller urged the extension of the list of principles as far as possible, and the exemplification of each at various levels by a number of pertinent illustrations.

Dr. Spuhler expressed the hope that the Chairman would revise and complete his original document.

Finally, there was general agreement that the results of the Conference should be considered by the Biology Council, and its further dissemination determined by the outcome of the Council's examination.

The Conference on Concepts of Biology closed with an expression of appreciation, from the Chairman, for the time and effort contributed by the participants.]

Appendix I

OUTLINE OF SUBJECT MATTER, METHODS, AND LEVELS OF BIOLOGY

Appendix II

UNITS AND CONCEPTS OF BIOLOGY[1]

R. W. GERARD[2]

In this conference, which is concerned with the concepts and units of all science, my assignment is the sector of biology. The goal is thus set to consider the life sciences in the context of all science—to compare and contrast, with attention to both similarities and dissimilarities. An approach that is too general will lead into the problems of philosophy; one that is too particular, to the separate subdisciplines. Attention to the sector boundaries, or junctions, thus seems the most efficient, and I shall therefore emphasize the boundaries between physical and biological science and between biological and social science. Since the former boundary has had far more attention than the latter, and since differences have been noted more vividly than have similarities, my emphasis is on biosocial comparisons, a topic that has occupied a portion of my effort in recent years (3, 4, 9, 10).

ENTITIES

It is not chance that the cleavage between natural and social science is greater than that between the sectors of natural science; it is a cleavage between substance and action, body and soul, the objective and the subjective. Inquiring man scans the universe with his sensory end organs, orders and classifies the information thus obtained, and so imposes a structure on the world he recognizes. Here is William James' "blooming, buzzing confusion"; here, in Henry Adams' figure, is man "on a sensuous raft adrift in a supersensuous sea"; and here is the impact of Kronecker's dictum on mathematics, "God made the integers, man did all the rest." Some inhomogeneity must exist for man even to be, and emphatically for him to divide his world into classes. And, since man depends mostly on visual information (two-thirds of all the nerve fibers that enter the human central nervous system come from the eyes), and since the eye detects primarily patterns of spatial extension, man first sees his universe as a collection of material objects. An entity is distinguished from its ground and, given appropriate duration, an individual is born to the perceiver. This is the basic event.

The individuals that people man's primitive world are necessarily commensurate with his own dimensions, his sensory range, and his time span; indeed, they even seem to conform to his status as a living being, for they are strongly personified. As technology offers instruments that reveal the lesser and the greater, as man's senses are extended (and mainly, again, his vision), new entities engage his attention; but this is a later development (2). More immediately, various observed individuals are recognized as having some common attributes and so as being amenable to grouping or classifying. This is the taxonomic stage of knowledge, and it follows the stage of simple observation and description just because differences are more likely to command attention than are likenesses (5, 6). Man thus types his observed concrete entities into sets and, as the second abstraction (the first being entity from ground), draws sharp boundaries about them.

But, as Whitehead well said, "Nature doesn't come as clean as you can think it";

[1] Reprinted, with kind permission, from *Science*, 1957, 125, 429–433.

[2] This article, prepared explicitly for the AAAS symposium on "Fundamental units and concepts of science" that was held on December 27–28, 1956, nonetheless rests heavily on the conference on Concepts of Biology (and on work with colleagues at the Mental Health Research Institute) and can serve to help integrate the rich discussion of the conference.

and, with growing sophistication, man replaces his plateaulike typology with the graded slopes of a probability distribution in a population of nonidentical individuals. A mere collection of seemingly unrelated entities is first given meaning or pattern in terms of perceived similarities; only later is it possible to look more closely at the individuals and to reintroduce differences, but now ordered differences with significance to the larger whole. Moreover, once the initial integration (or induction) has been achieved, progressive differentiations (and deductions) can be meaningful, and subclasses can be conceived and identified, later to become graded subpopulations. This is a sign of growing familiarity with the entities of attention, indicating more interest and ordering and leading to subdivision of effort, or to fragmenting of science. Attention to a subject matter reveals finer differences and new attributes, first of whole individuals and then of their parts and structure, calls for new words to characterize these, and adds new digits to a decimal number as subclasses and subsubclasses become significant. Here knowledge is in the morphologic stage.

So far, we have considered primarily material entities and their grouping on the basis of sensible, mainly visual, attributes. Clearly, animate and inanimate objects are more alike than are objects and the behaviors of objects; so the physical and life sciences, concerned (as a first approximation) with these two types of object, are closer to each other than they are to social science, which is concerned (still as an approximation) with the behavior of one variety of animate object. But the real shift here is from a focus on organization to a focus on action, from being to behaving, from form to function, from pattern to process, from the timeless to the temporal. "Being" is the cross section of an entity in time, and those aspects of the organization which appear relatively unchanged in a series of such instants constitute the essential structure of the entity or organism. Invariance in time helps to identify the significant units of a mature system. Conversely, along a longitudinal section in time appear the transient and reversible changes, often repetitive, that constitute "behaving" or functioning, and the enduring and irreversible changes, often progressive, that constitute "becoming" or developing. And with this shift in orientation to time there occurs a shift in the entity of concern—from an object, a pattern of matter in space, to a behavior, a pattern of events in time.

Let me briefly recapitulate. Man's attention is first drawn to particular objects in his experienced world. These objects are grouped into classes and subdivided into components, at first with Procrustean rigidity and later with more freedom of variance, and then interest shifts to particular processes. But, as a group is more removed from concrete prehension than are its component entities—a species seems more abstract to us than does an individual organism—and as its component entities become more or less interchangeable, so is a process more abstract and universal than are the acting objects. Permeability, to some thing—ion, gene, idea, person—and in some degree, is a property of all boundaries (indeed, a boundary may be characterized as a zone of lowered permeability), as irritability, quantified as a threshold to some environmental change, is a property of all responsive systems. This shift in approach is like that from phenotype to genotype or that from observation to model building.

For study of permeability or irritability, the particular system examined is not initially so important as is what is done with it, and the common denominator shifts from the object to the variable. Some attribute of the object is selected for attention and measurement as the object is manipulated. New methods arise, experiment dominates observation, and changes of the observable in time are almost universally examined. Not the nerve but the nerve impulse is the entity of concern, and this can be studied on any particular nerve that comes conveniently to hand. True, important differences appear between impulses in invertebrate and vertebrate nerves, in nerves of the frog and man, and

even in separate fibers of a single nerve; but these differences are mainly quantitative ones in the same parameters. Indeed, the behavioral similarities are so significant that eventually a functional criterion may help to define a material set: a nerve cell is one that conducts an impulse, and a smooth muscle cell is sometimes best distinguished from a connective tissue cell by its ability to contract. This is the essential shift from the morphologic to the physiologic mode. It must not be forgotten, however, that manipulation remains limited to the material object: the influence of temperature on conduction rate is determined by warming a nerve, not a nerve impulse; the answer to the question, Did you ever see a dream walking? is *No*.

The pure morphologist, then, is concerned with the structure of particular objects and attempts to make his description ever more complete. Here is the gross anatomist and naturalist of the past as well as the old organic chemist describing a substance or the visiting anthropologist describing a village. (The electron microscopist or cytochemist or ethologist of the present, as well as the modern macromolecule chemist or the factor analyst seeking primary abilities or the sociometrist noting contacts or quantifying opinion, is often busy with the specific case but is usually concerned really with the class.) He observes what is; and he seeks ever more powerful tools to identify a system and fix it at an instant of time, to reveal its finer detail, to discriminate its more subtle differences, and to do this more precisely on more limited samples. His concern is primarily with the individual instance, like the clinician's with his patient, the humanistic historian's with his character or period, the artist's with his poem or painting or other unique creation of man. When a class property becomes the focus of interest, comparative studies replace those of the individual, and descriptive morphology gives way to comparative morphology or systematics or physiology or genetics or some other discipline concerned with relation or function or development. As the class or property replaces the individual—as the actuarial approach replaces the clinical approach—there is greater distance between the operator and his material, the material becomes more objectified—not Tabby, but a cat; not John and Mary Smith, but a family—and analysis is added to description.

The entities or units which are significant, which are invariable (organization) or repetitive (function) or progressive (development) in time, similarly shift from concrete material entities to abstracted properties of classes of material entities.[3] It is an interest in such attributes as personality traits or social roles and connections as the units of structure (aside from personal behavior and role-playing as actions), rather than in the individual or groups that exhibit them, that makes some aspects of social science seem different from life science.

LEVELS

An entity of interest, then, may be an object or some property of it or a class of objects or some property of the class. But the class is, of course, a kind of individual; and the more the members of the class interact—even to the extent of developing into differentiated subclasses—rather than coexist, the more does the superordinate group become a true individual rather than a collection of ordinate individuals. The shift from separated cells to a reproducing body, shown by the slime mold, remains an excellent example of such individuation; a species with interbreeding individuals determining the properties of its gene pool

[3] Much confusion has arisen from the use of a given word as a noun, a structural connotation, and as a verb, a functional one. Thus, *function* in physiology, *adaptation* in systematics, and *role* in sociology, as examples, when used as nouns, refer to an existing state; as verbs, to an action. The state, in each case, carries an implication of purpose and value, of the org at one level of the superordinate system; and the action, similarly, implies a behavior of the unit that has utility in the larger setting. Adaptation of the individual, in adaptive amplification, is different from the adaptation it has acquired in an environmental situation; the adaptive radiating of a population is different from the adaptive radiations of a phylum.

is a continuing superordinate individual. This is the now widely accepted relation of hierarchy and levels (8). The atom, an individual or unit, is built of subordinate differentiated and interacting units, the various nucleons and other ultimate (for the moment) particles, and is built into a superordinate molecule. The individual molecule, in turn, with like or unlike fellows, becomes a crystal or a colloid or some other material aggregate; the colloids form particulates; these, cells; cells, tissues and organs; these, organisms; and organisms, species and larger taxonomic categories, or, in another way, groups, communities and larger ecosystems. I have found the word *org* convenient for those material systems or entities which are individuals at a given level but are composed of subordinate units, lower level orgs, and which serve as units in superordinate individuals, higher level orgs (1). The important levels are those whose orgs (entities) are relatively enduring and self-contained. Thus, a cell is more likely to continue as an individual maintained entity than is a given colloidal micelle or a cell particulate, and an organism will vary less in time than will its parts or organs.

In the course of cosmic evolution, assuming a start in chaos, orgs at a given level become more highly integrated—with a clearer boundary, with more differentiated and interdependent units, and so with a more complex structure and more powerful regulating mechanisms—and new levels are superposed. But each added level permits the combination of old level orgs, now subunits of the new org, in various ways. It is because of the explosive increase in richness of pattern with rise in level that there appears to be an emergence of unpredictable novelty. The particular org that forms is indeed understandable in terms of the units, and their relationships, of which it is built; in this sense the situation is reductionist. But the particular org and its properties are rarely predictable a priori, because of the great number of possible outcomes, with either known or unknown probabilities and with that strong dependence on unspecified values of unidentified conditions which we call chance.

Thus, higher level orgs are likely to have a greater variety than lower order ones, and they are likely to depend more on their particular past; they are more individual. But they are also less plentiful, since several subordinate units contribute to each. A handful of ultimate particles form a hundred species of atoms, or perhaps a thousand, noting isotopes, and a hundred kinds of atoms form millions of molecular species. But the total of molecules is less than the total of atoms, and the total number of members of an average species of molecule is far less than of the average species of atom. The kinds of living organisms compoundable from the subpopulations of atoms and molecules (and combined or macromolecules, as nucleoproteins from amino acid and nucleotide building blocks) must be infinite in any meaningful sense; and the groups compoundable from organisms must be even more so. Yet the number of members of each kind of organism is vastly less than of each kind of molecule; and of groups, again less. In fact, whether from insufficiency of material or of time, the actually realized species or organisms are probably fewer than those of molecules (certainly they will be with the creative meddling of synthetic chemists); and the realized groups or ecosystems or societies, far less again.

Certainly all realizable molecules or cells or organisms or groups have not been realized, and the null members are not distributed at random. Particular combinations, orgs, are more stable in a given environment than are others, and these will be "successful"—that is, will occur in larger numbers—while that environment lasts. Furthermore, individual orgs form sets, as species form genera (and elements thus fit into columns of the Mendeleev table); molecules divide as to ionization or polymerization or what not into inorganic, organic, and macro molecules; organisms classify into kingdoms and phyla and down, because of a kind of periodicity in the patterns of formation. And, the grandest

dichotomies of all, the hierarchy of levels has branches. The physics of atoms is unitary. The chemistry of molecules is strained between inorganic and organic and bio but still retains a unity.

At the next level, however, is an unquestioned split into the complex inanimate orgs of geology and astronomy (and meterology and oceanography, perhaps even of architecture and engineering) and the complex animate orgs of biology. Physics and chemistry thus are subordinate to biology, the earth sciences coordinate with it; and in many ways the earth sciences are more comparable to biology than are the former. The entities of concern in biology and the earth sciences, as compared with physics and chemistry, are more individual and there are more species of them to deal with; and as unique orgs at the supermolecular level they are closer to ourselves, are more a subject (*thou*) than an object (*it*).

In the domain of biology between the cell and the multicellular individual, the tissue is an org with cell units, and so also is an organ. The first is a population, with cells related by origin or history, and a loose org; the second is a tight well-integrated org, with cells related by function. The same threads extend beyond the individual—to species and larger population categories, based on descent, and to ecogroups of various sizes and levels, based on fundamental interrelation. Moreover, above the level of the individual occurs another major branching, with societies—especially but not exclusively of man, or even of any single species—diverging from the population axis. Here population genetics and systematics leave ethology and ecology, and here social science separates from biology. The ecosystem of a lake or forest, or of an ant hill or flock of starlings, is thus coordinate with human spatial and functional groups, the village or the tradesmen, as is the clone with the clan as a lineage group. Again there is a jump in individuality and a diminution in kinds, a greater nearness to man and a consequent shift from object relationship toward subject relationship, at the social level.

BECOMING; HISTORY

History, or becoming, I said in a preceding section, is a regular change, normally progressive, in a system along the time axis; function, or behaving, is a repetitive perturbation along this secular trend; and structure, or being, is the instantaneous status. The units and subunits of an org are nodes of stability, relatively constant in time. These are the structural residues of past action, the molecules or organs or institutions that have become fixed, yet which also carry the cumulative changes of becoming. It is critical, however, that, whatever role process initially played, traces of the past can be carried forward only by concrete material entities, not by abstracted units (7). The neurone can evolve or develop by changes in its components, and so the nerve impulse can also change, in speed, intensity, and what not; but the impulse, per se, does not develop; it is a single action and has no history. So also, the individual person—or generations or groups of persons—carries the history of a society, even though the role or status is the unit of interest. And, of course, the gene—or generations and arrays of genes—carries the heredity of the organism and the population. It is well to note, to prevent confusion, that secondary material products of the primary entities may also be carriers of the information and ordering, the amount and kind of matter, which the past imposes on the present—wooden vascular tubes, seed cases and egg albumen, chitin or bony skeletons, elastic fibers and plasma antibodies, nests and burrows, buildings and machines, books and recordings, are examples—but these separate material carriers only reemphasize the point.

In the becoming of a given entity, there may be a shift in emphasis from one carrier of the past to another, and the shift is normally up the hierarchy levels. A gene, if it is a nucleoprotein molecule, is the product of vast chemical adventures, from the formation of atoms and simple molecules in the distant past of its ancestral

lineage, to relatively minor shifts in kind or arrangement of atom or radical, the mutations of its macromolecular maturity. The cell is directed in its development, first by the information stored in its genes, later by the structures and substances that have been formed partly under their influence—reduplicating particulates, somatic mutations, adaptive enzymes, and the like. The organism, in turn, develops by virtue of the various cell types that are differentiated early in its individual existence and their later patterning and other modification as tissues and organs. And the group, finally, changes as its component individuals learn differential roles and skills. It is hardly surprising, then, that higher level orgs are more individualized than lower level ones, that they are more determined by their particular experience, and that they carry a richer and more characterizing past. A society becomes what it is through learning by its individuals, morphogenetic development by their cells, reduplication with mutation by their genes, and so, by regress, into the domain of chemistry.

Attention was focused, in the preceding paragraph, on the units as carriers of the past. Equally essential in shaping each present from its immediate past is the environment acting on the unit. Indeed, at each stage of development of an org, the heredity is fixed in the units entering that stage; and the environment, interacting with these units, leads to new fixations—irreversible changes—in these units or in superordinate ones. Thus, of course, arises the progressive specification and differentiation of orgs, an amorphous totipotentiality yielding to a concrete realization. And the magnificent inventions of gene reduplication and recombination, of heredity and sex, insure stability with variation; as the environment, operating through mutation and selection, insures guided change. At other levels, the mechanisms of becoming are less understood; but there is little doubt that they are similar in broad principle, dissimilar in all else.

Since at each stage and level future development could be along any one of a number of branching paths, depending on the vicissitudes at the moments of decision, the difficult problem is not that of diversity but that of uniformity. More than 40 cell generations lie between human egg and baby, and at each division a slight difference in cell properties or arrangement could magnify through subsequent ones; yet billions of babies have been born within the fantastically narrow range of "normality," only a negligible scattering of monsters outside of it. Of course, too great an abnormality cannot continue its development and is cut off by death; but, aside from such selection, there are self-regulating or homeostatic mechanisms in operation at all times and levels. If enzyme molecules are too active, a fall in substrate concentration and an increase in end-products will slow the reaction. If cells multiply overrapidly, they become too far removed from a source of nourishment and are retarded. If a liver is lagging in its many functions or a nerve trunk is failing to innervate its peripheral field, the structures will grow or regenerate—controlled by still unknown mechanisms—until performance is adequate. Populations of predators and prey regulate each other in quantitatively predictable ways. And if a man deviates far from the norm of his culture, social pressures and sanctions—by better understood mechanisms than the morphogenetic ones—bring him into line or exclude him. Homeostatic processes nudge orgs toward a uniform state. The interaction of units to form a superordinate org is regulated, as is their action to maintain it.

BEHAVING; REGULATION

This viewpoint has been little applied to the secular changes of long-range becoming, but it is the daily bread of moment-to-moment behaving. The vast bulk of the functioning of any enduring system is as displacement-correcting responses. Here is the negative feedback of engineering or the adaptive or self-regulating or homeostatic response of physiology. All orgs maintain themselves in a dynamic or flux equilibrium by mobilizing internal reserves to oppose environmentally imposed change; or, more rigorously, each unit

responds to loads imposed on it by its environment (which may be the superordinate org of which it is part) with responses of its subordinate units that tend to eliminate the stress on the whole, even at the expense of a greater temporary displacement of the part. It is in the particular mechanisms and sequences that different orgs, and especially different level orgs, differ from one another; and each case must be examined individually, as for its structure. Yet here also important commonalities exist.

The organization of an org, its function-structure complex, is investigated by imposing displacements on it. Ordinarily an input is presented, and the output is observed, the stimulus-response situation. But the thruput in the system can also be manipulated—as in plucking a piano string, stimulating a neurone pool or cutting a nerve tract, or blocking an artery or a highway—and the spread of, or adjustment to, the disturbance tells much about the system. The quantitative relation between magnitude of displacement and strength of restoring influence—linear, concave, convex, sigmoid, or more complex—as also the existence of different or like mechanisms for restoring displacements from opposite directions, and whether the return is oscillating or damped, might serve to group widely different orgs into classes. There is a limit of homeostatic tolerance, an amount of displacement of a system beyond which it will not return. Change is then irreversible, and process leaves behind it structure —behaving shifts to becoming and alters being, sometimes leading to pathology and dissolution.

General questions can also be asked at this level about the degree of displacement tolerated in relation to kind, repetition, frequency, direction, and other aspects of the load; about the safety factor; about the speed of change of physiological zero, or adaptation; and about many other matters. Moreover, since structure is a product of history, or irreversible process, the character of the material change can serve as an index to properties of the action. A highly regular structure, as a honeycomb, indicates a highly determined process, even though this is a behavior of a group of organisms. Striated muscle fibers are highly ordered longitudinally and more variable in section; presumably the micelles are arranged very powerfully, once formed, but the number in a fiber is determined more by chance.

BEING; ORGANIZATION

An organism has organization, an ordering of material in space and of events in time. Any random arrangement is an order; the essence of ordering is that some particular order, out of all possible ones, will be produced. The particular one can be defined in relation to the observer, as near and far; or to some polarity he chooses, as large and small; or to a functionally related object, as key to lock; or to a generatively related object, as parent to offspring; or to an unrelated object, as a photograph or model to the original. Of these, the ability to reproduce itself, along with any fixed aberration, is the most demanding and is especially characteristic of biology; and life has been defined as "the repetitive production of ordered heterogeneity." The guiding information is carried, and the given arrangement is imposed or reproduced by various means, from electric fields around linked pyrimidines in nucleic acids (four of which can "code" the building of the 20 amino acids of proteins), through the protein antigens of cellular immunity, the metabolic and allied gradients of morphogenesis, the engrams of racial or individual experience, to the coded tapes of calculators and the culture traits, especially language, of civilizations.

Communication of information across org boundaries, between entities at the same or different levels, is not only the means of fixing the past; it is also the means of responding to the present. Nerve impulses and hormones, like talk and books, are transient or more enduring signals (or symbols). Perhaps hearing is more important than vision to social man, as is often claimed, because speech is the vehicle for the immediate communication of information in ongoing interacting behavior. Indeed, a major difference between physical,

biological, and social orgs may be in the relative importance for them of the energy, substance, information, and meaning that cross their boundaries. The higher the level, the more do individuality and specificity enter and the more is the system coded to, or discriminating of, differential environmental stimuli or information.

The more also does the study of higher level orgs involve the use of experimental methods and mental tools dealing with patterns of relatedness. The forking paths of a nerve impulse through a brain, or an infection in a population, or a rumor in a community reveal connectivity patterns; and for the analysis of these relations of "organized complexity" are coming to hand the new techniques of set, game, and probability theory, of topology and stochastics, and of other nascent branches of mathematics or logic.

History produces structure, and structure determines function—becoming gives being, and this is capable of behaving; order is produced and maintained—but the relations are so intimate and seemingly reciprocal that the distinctions sometimes seem artificial. Further consideration shows that this is not the case. For the function of an org at its level depends on the structure of its subordinate units, and the structure of these subordinate-level orgs depends, in turn, on the history of their sub-subordinate units. Contraction of a muscle fiber is possible by virtue of the fibrillar and membrane structures, and these are produced by processes involving macromolecules, enzymes, and other submicroscopic units. It would lead too far afield to develop the notion, but it deserves thought, that history, structure, and function stand in relation to one another as do cause, org, and purpose. In both triads, time runs, say, from left to right through past, present, and future; and levels rise from left to right through subordinate, ordinate, and superordinate. Incidentally function (the noun), with an overtone of duty, relates an ordinate unit to a superordinate org; at its own level, functioning has an overtone of pleasure.

CONCLUSION

In the remaining space, I can merely suggest the concrete application of these considerations. Again a brief recapitulation. The units of man's attention are first concrete objects, directly sensible. These are classified, then seen as populations with variance; dissected or combined, to sub- and superordinate units forming hierarchical levels; compared and analyzed so that functional units replace or add to structural ones; and considered in relation to time, both as to irreversible development and to maintenance or restoration of equilibrium; and in relation to order and the information carriers that reproduce it. The world of organized experience thus plots on a map, with orgs at different hierarchical levels—molecule, cell (or crystal), organism, group, population (or society)—along the ordinate; and with their properties—becoming, being, behaving—along the abscissa. The entities, the disciplines concerned with them, the manipulative and rational methods for studying them, and the resulting concepts about them, can be classified into appropriate squares of such a table.

The hierarchy has two major branchings: (i) above the molecule level, into more organized entities with or without the collective properties that describe the living; and (ii) above the organism level, into entities based on human or non-human components. Biology is thus superordinate to physics and chemistry and, at its lower levels, coordinate to the earth sciences; it is subordinate to and, at its higher levels, coordinate to the social sciences. The boundaries are reasonably sharp; yet the biochemist or biophysicist or electron microscopist, concerned with molecular traffic and macromolecular edifices, is much closer in attitudes and operations to the physical scientist, while the systematist or population geneticist or ecologist, concerned with organism traffic and population edifices, is much closer to the social scientist, than these different-level biologists are to one another. And perhaps the biologists operating between cell and organism levels are most akin to, say, meteorologists and might

find rich mental nutrition by learning how they handle such problems as storms by the study of individual hurricanes, from Alice to Zelda.

The attributes that help define living orgs are (i) highly ordered and clearly bounded heterogeneity, spread over many levels and with many differentiated units at each; (ii) dependable mechanisms for reproducing units and patterns, by reduplication of the information carriers, and for altering them, by recombination of carriers and by the innovating (mutagenic, imprinting) and selective action of environment on the carriers; (iii) powerful homeostatic mechanisms for maintaining and regaining equilibrium, including especially the use of transported material, transmitted activation by energy or signs or signals, and stationary dominance-subordination gradients. The gene, materialized as a macromolecule, and the idea, materialized as an engram, chiefly among the transmitters of enduring order, are the bearers of structural and behavioral heredity; they carry the past of the entity and account for its individuality. The hormone and the nerve impulse and the sound or gesture, chiefly among the transmitters of transient order, are the bearers of information and instruction to and from orgs or their subsystems and evoke adaptive or innovative behavior that maintains the entity or modifies it in conformity with environmental pressures.

The student of the living stands between the students of the material and of the human on an ordinal scale. He deals with entities or orgs or systems that are less when compared to the latter, but more when compared to the former; more various and more individualized: more highly ordered and capable of more varied behaviors; more dependent on a particularized past and a discriminated present and so on fixed or transient information; more devoted to self-maintenance and self-duplication over the short run—by stability, supplied by feedback and inheritance—and more devoted to change and adaptation over the long run—by modifiability, supplied by learning and gene shift and guided by environmental selection; more sensitive to more environmental variables and more able to dissociate the response from the stimulus in magnitude, kind, and time interval; more personified and closer to the observer and harder to dimensionalize and quantify; more "free" to achieve their "purposes" and reach their "values," including survival and "progress," and to be "aware" of the attendant experience of inner "private" and outer public "reality"—if these words add anything to what has been said.

Such an exercise in analysis and integration is more than an exciting mental adventure; it can have important and useful consequences. The attention of an investigator may be directed to other disciplines from which ideas or skills or information can be plucked ready to apply to his own. Social scientists have been slow in exploiting biology in this way, but they could profit much from its approach and content. Acculturation as a stress, culture as a self-regulated internal environment, institutions as organs, ideas as heritable social mutations and subject to the same factors or pressures as operate for organic evolution, ideologies as polar or balanced views of man as a whole entity and man as a unit in a larger unity—such viewpoints can demonstrably aid understanding of the social epiorganism or the body politic.

The interrelations of subdisciplines in an investigator's own field may be exhibited, so that the great unities are not lost in the small particulars. This may spark the seminal insight that leads to a new structuring of the universe of interest and, failing this, must reveal areas of research emptiness or duplication. And this also should favor presentation of biology as a dynamic whole, with a few penetrating concepts replacing a legion of detailed facts and words, to our students and to our public. Life science is a great entity, and part of a greater one; biology, all science, will attract more and better members and more generous and enthusiastic support when, in all senses, the forest is added to the trees.

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Appendix III

SELECTED REFERENCES

This collection of 100 titles on concepts of biology was selected both to represent some of the literature on theoretical biology and to point to further discussions of some of the topics mentioned in the Lee Conference. About one-third of the titles were suggested by other members of the Conference. Some other compiler might with equal or greater reason choose half of the titles differently. A few elementary works, some designed as textbooks, are included because they present substantive aspects of biology in a context which stresses conceptual matters. Works in English are given priority as are those of later date. A few titles not generally regarded today as "good" are included because of unique coverage.

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NATIONAL SUPPORT FOR BEHAVIORAL SCIENCE

February 1958

The present statement grows out of widespread concern that the national interest be adequately served by the development and application of behavioral science. This concern was recently voiced by various government officials and behavioral scientists in informal discussions. As a consequence of the expression of urgency, a group of behavioral scientists has prepared a brief set of observations and recommendations.

This small group, whose members are from many different sectors of behavioral science, came into being voluntarily in response to the expressed interest of government officials, and goes out of existence with this statement. We speak as individuals and not as representatives of any institutions or associations. Time available did not permit a canvass of professional opinion or a thorough survey of existing resources. It was not our objective to make this report comprehensive. We focus on behavioral science and obviously cannot touch on all pertinent fields. Moreover, our examples of research tasks are just that—intended to give concreteness and image to representative problems rather than to be a firm selection of research priorities.

We hope that this document will stimulate further thought and prompt action, equally on the part of our professional colleagues, government officials, and citizens generally.

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SUMMARY

The present situation facing our country calls for an evaluation of the role and potential contribution of behavioral science. This is the combined endeavor of many fields investigating all aspects of behavior, leading to understanding of human beings as individuals and in social relations. Behavioral science therefore includes many studies in the fields of anthropology, biochemistry, ecology, economics, genetics, geography, history, linguistics, mathematical statistics, neurology, pharmacology, physiology, political science, psychiatry, psychology, sociology, and zoology. Applications ramify into advertising, business administration, education, government, human engineering, labor relations, law, medicine, military science, operations research, personnel selection, public relations, and many other aspects of human endeavor. Some of these sciences are still in early stages of development, but American research in them at the moment has a clear lead over Russian, which is constricted by Communist dogma.

Behavioral science has demonstrated its usefulness to human welfare and national security. Its further development could increase its contribution in areas of international relations, military defense, and national vigor.

To accomplish these goals, the following recommendations are offered:

I. Formation of an advisory panel of behavioral scientists to work closely with the Special Assistant to the President for Science and Technology. There is need for more understanding, backing, and use of behavioral science throughout the government and by the people of the United States, and for encouraging the scientists themselves in their research tasks.

II. Provision of increased funds for behavioral science research, training, and facilities in the National Science Foundation, the Department of Defense, the National Institutes of Health, the Atomic Energy Commission, and other appropriate governmental and private agencies, in order to:

A. Establish additional university programs or institutes to conduct research in designated crucial areas.

B. Finance more fellowships, both predoctoral and postdoctoral, especially in all the social sciences.

C. Increase financial support for basic research in behavioral science.

D. Make available special facilities for behavioral science, including buildings and equipment.

E. Support centers devoted to foreign area studies.

I. NATURE OF BEHAVIORAL SCIENCE

The United States finds itself today in a world situation which demands assessment of every resource of physical, intellectual, and moral power. In this memorandum a group of citizens examines the present state of behavioral science and how it can improve international relations and foster national intellectual and moral power. We identify areas in behavioral science where acceleration of understanding is feasible, and where application of such knowledge to our problems is crucial. Coming from divergent fields with necessarily different points of view, we nevertheless share unanimous conviction concerning the significance of these areas for human betterment and the urgency of expanding their support.

Behavioral science applies the methods of science—examination, measuring, testing—to man himself. As medicine draws from many sciences whatever knowledge concerns the health of people, so behavioral science draws from many fields what each can contribute to the central problem of understanding the behavior of man. It studies the actions of men, their relations to each other, and to nature.

Behavioral science, striving toward, but not yet attaining, unified theory, seeking precise quantification and accurate prediction of man's behavior, is young, and its subject matter is complex. We know more about heart disease than we do about mental illness. We know more about the laws governing the orbits of planets than the laws governing action and reaction of

human groups. Many methods of behavioral science are still crude, yet much is known which, experience has shown, can be put to good use. Much behavior can be described systematically, and changes produced by altering features of the physical and social environments can be predicted. Sometimes a scientific approach pays off well, but to most practical problems the new sciences candidly admit they cannot give immediate solutions. Yet they may suggest the right questions to ask or indicate in what directions the answers lie.

Russia at present lags behind the West—particularly the United States—in the biological, psychological, and social sectors of behavioral science. The biological and medical sciences in general have not been prosecuted in Russia with the zeal of some of the other natural sciences. Marxist doctrine has held back the social sciences, and an adherence to Pavlovian doctrines has retarded much of Russian neurophysiology and psychology. Political considerations have also limited objective investigation of human genetics.

In some behavioral areas basic Soviet science has been adequate, and in other areas research has been successfully applied, as in pedagogy. In general, however, undue stress upon immediate application has sharply restricted fundamental behavioral research.

The Soviet Government has interdicted the use of psychological testing methods and attitude surveys, methods most useful to democracies in probing the opinions of the people in order to aid government and planning activities. Also, ironically, behavioral science has been used least in the Soviet Union for precisely those purposes for which Americans popularly think totalitarian states would use them most: political propaganda and the control of human behavior for political purposes.

Though the West leads in accomplishments in behavioral science at present, and has more and better trained scientists, the fact that this field is at an early stage of development, here and in all other nations, means that we could be surpassed by a country which concentrated serious effort to that purpose, if we do not intensify our own efforts.

We must assume the probability of a breakthrough in the control of the attitudes and beliefs of human beings through exceptionally effective educational techniques, drugs, subliminal stimulation, manipulation of motives, or some as yet unrecognized medium. This could be a weapon of great power in Communist hands, unless comparable advances in the West produce effective countermeasures.

The democratic conviction of the dignity of man and his role in society is a point of view that stimulates development of behavioral science. Concern for the individual has traditionally set problems for philosophers, statesmen, and spiritual leaders. Human behavior represents a complex intermingling of moral and physical considerations, and there is an important region of overlap between behavioral science and the humanities, particularly history, languages, literature, philosophy, and religion. The sciences of man often build on and clarify the wisdom of the humanities, and attempt to formulate and test precise laws.

Before World War I it would have been difficult to point to many applications of basic research in behavioral science that were better than the common-sense judgments of competent individuals. Now there are instances in which basic research has led to important advances that have already had impact on society. These include: intelligence and aptitude testing; techniques to speed learning and increase the effectiveness of education; use of drugs in alleviating or curing certain mental illnesses; sample survey methods using mathematical statistics for measuring and predicting social trends; development of increasingly reliable economic indicators fundamental to planning in government and industry; use of group dynamics to improve the efficiency of face-to-face working groups; and many others. Such achievements make for widespread general acceptance—sometimes too enthusiastic and uncritical—of behavioral science.

Throughout the ages when man has gained more knowledge he has gained more

freedom. His understanding of physical laws increasingly has enabled him to control the natural environment, protect himself from heat and cold, and travel speedily and comfortably. Expanding biological science has freed him from pestilences, most of the terrors of childbirth, the feebleness of malnutrition, many forms of pain and physical misery. His life span has been prolonged and made more healthy.

Similarly behavioral science, directly probing man's central nature, gives promise of increasing his degrees of freedom and expanding his effectiveness and creativity. He can be released from the constriction of life by neurosis and feeblemindedness and the tragedy of psychosis; the limitation of opportunity from inadequate education, associated prejudice and bias; the diminishing of contentment and effectiveness from marital strife, industrial unrest, crime and delinquency; and perhaps most of all, the fear of international conflict which constrains the free expression of the world's peoples. To these central problems of human existence the sciences of man ultimately address themselves.

We are coming to accept as likely in the future a sort of physical existence which would have been unimaginable a few years ago. But the potential benefits which can flow from basic study of our behavior are not so clearly seen. A chief purpose of this report is to present and urge these bright prospects.

II. ILLUSTRATIVE FIELDS OF BEHAVIORAL RESEARCH

A few examples of the many areas of basic research which can be applied to bettering human life are:

a. Drug Effects on Behavior. Beginning with the sedatives and anesthetic drugs, and dramatized at present by new tranquillizing and anti-depressant compounds, the field of psychopharmacology is developing with vast potential importance for understanding behavior. Chemicals are involved in brain activity and hormones affect behavior; compounds exist which can produce temporary psychotic states or lower resistance to communication of confidential information. Certain drugs, when injected into animals or man, can modify sexual behavior or subjective emotions. The fundamental significance of all this is that clues are rapidly unfolding which give us entirely new insights into the biological determinants of actions, feelings, and reasoning processes. The wide range of possible industrial and medical uses is apparent.

b. Creativity. Among the highest traits is the ability to produce new intellectual discoveries and artistic products. Such novelty invests life with richness. Methods are being developed for investigating the creative processes of geniuses and others of unusual ability, as well as the effects on them of early experiences and other influences. Quantitative techniques are being worked on for measuring and analyzing inventive processes of discovery. The conditions under which creative activity flourishes and the role of emotions in aiding or blocking it also are being investigated. All this work can lead to expansion of horizons of satisfaction for the artist and creator as well as for those who profit from their endeavors.

c. Human Performance Capabilities and Limitations Under Stress. While the capabilities and limitations of human beings under normal conditions of work have been extensively studied, little is known about them under extreme work conditions or stress. Nor is much known of techniques of training, motivation, environmental aid, or pharmacological support for individuals who must be required to undergo extreme conditions while maintaining high proficiency on the job. Examples of such extreme conditions are: long stretches of continuous confinement in atomic submarines or space vehicles; absolute or relative isolation of individuals or small groups; pressures of responsibility in high governmental office; extremes of tropical heat or arctic cold; enemy-applied insults to body and mind; and information input overload. Basic research effort is needed to explore not only the limits of human capability under stress, but also the techniques that may be used to improve performance under such influences.

d. Personnel Utilization. Much research in behavioral science has been and can be done on the problem of putting the right

man in the right job in the armed forces, industry, civil service, or elsewhere. Such investigations define the human tasks to be performed, the jobs and organizations of men required for the accomplishment of those tasks, the kinds and numbers of men that must be recruited, the types and amounts of training they must be given, the standards of performance that must be met, the differentiation of careers that must be offered, and the criteria for the promotions, separations, and reassignments of men that must be effected. Psychiatry and psychology have made major contributions by providing instruments for the selection and classification of men in terms of aptitude, by defining effective techniques of training, by producing improved measures of job performance, and by providing systematic procedures for the anticipation of human skill requirements in new and untried situations. Social psychology and sociology have made contributions to the solution of problems of human relations, morale, leadership, and the management of human organizations. Nevertheless, the full potential of these sciences for improvement of the effectiveness and economy of management and administration in all fields has not been realized.

The demands that will be placed on military and civilian personnel management systems of the future cannot be fulfilled without full application of behavioral science. The industrial or military organization of the future will require highly selected, thoroughly trained, and technically skilled men who most often must operate as members of closely knit teams in conjunction with extraordinarily complex machines. In the most crucial of their activities, they must perform their functions with precision and wisdom of decision. Further, in military situations they may have to act under the stresses of isolation, vigilance, and unusual environmental conditions, operating in dispersed locations and in small groups in which problems of military organization and management, as well as leadership, will come in novel forms.

e. Decision Process. The role of man in the last half of this century will increasingly be that of an information processor and decision maker. Heavy demands will be placed on human beings at various levels—not only top administrators—for correct, closely integrated, vital decisions. The theory and empirical analysis of human decision processes have been vigorously pursued during the last ten years, with promising progress. However, gross deficiencies exist in basic understanding of the process, the conditions for its efficient operation in different contexts of information scarcity and information overload, and the conditions of efficient selection or training of individuals for decision making.

f. Group Functioning. Recent advances in behavioral science have made important contributions to our knowledge of man as a social being. Modern techniques of cooperation in industry, science, and warfare emphasize a greater reliance upon small groups of men functioning semi-autonomously and integrated into larger operations only through complex and sometimes vulnerable communication systems.

For a small group to be efficient, each member must contribute the particular technical and social skills expected of him. Basic research on group functioning will almost certainly yield important general principles for group assembly, training, and management, because a number of the technical tools and concepts needed for this effort are now available.

g. Measurement of Economic Process. Much of the strength of any society derives from its monetary policy and the effectiveness of its labor and management, its production, distribution, and consumption. Theory in this field has become more sophisticated with the years, and theories have been confirmed or destroyed by new measurement techniques based on masses of statistics, painstakingly collected. Recently new methods of econometric analysis of broad economic systems and economic decision making have been developed. Much that can profit national and international trade can be done to make this field more empirical and even experimental. Measuring the effects of psychodynamic factors on consumer preferences, of political boundaries on the flow of trade, of morale factors on fluctuations in stock values, and of nu-

merous other related elements can eventually put our understanding of economic matters on a basis of more precise science, and so aid in the effective mobilization of resources.

h. Cultural Differences and Change. The varied customs and different ways of life of the peoples of the world pose complex intellectual and practical problems. The extent to which these ways of life or cultures constitute systems as opposed to mere aggregates of customs is undetermined. If, as most anthropologists agree, culture are systems, perhaps the principles of their growth and decay can be scientifically formulated. Theory construction has begun and provisional generalizations are in sight.

The facts of cultural differences and rapid social change present difficulties to American programs of foreign aid and diplomatic or other communication with peoples abroad. Consequently, it is urgent that we develop a scientific understanding of our own and other ways of life. The use of surveys and other procedures of behavioral science can vastly improve our international intelligence activities. Many of the needed facts are already available in the Human Relations Area Files and other sources, but techniques for formulating sound theory and applying it in policy are still to be developed.

i. Man-Machine System Design. During the past ten years behavioral science has been employed with increasing effectiveness in the design of man-machine systems. It has influenced choices of components like dials and signals, levers and controls, and formulation of procedures to coordinate man and machine in accomplishing an integrated purpose. The advances in technology of mechanical components, such as electronic computers, however, have led to rapid increases in system complexity so that the available knowledge about the capacities and limitations of man in man-machine systems has become more and more inadequate. This is true for automatic manufacturing equipment, large-scale traffic control techniques, manned space vehicles, systems for the surveillance of combat areas, and those which automatically collect and process information. In part, this deficiency results from the fact that only primitive methods are available for comparing the effectiveness of systems in which human beings and equipment must be coordinated. In part, the deficiency is attributable to inadequate scientific information about man's perception, memory, reasoning, and decision-making capabilities.

The joint participation of systems engineers and behavioral scientists in basic research on man-machine system design and use, and the provision of adequate system simulation facilities for such purposes, are necessary for advance in this field beyond present rule-of-thumb methods.

III. PERSONNEL AND FINANCIAL SUPPORT FOR BEHAVIORAL SCIENCE

Behavioral science has grown principally in the universities rather than in applied or professional settings. Well established graduate training programs are found in 40 to 50 American universities, and at least 20 of these may be judged clearly superior.

The number of talented persons who enter and complete graduate training in behavioral science is now limited by the shortage of fellowships and assistantships. A recent report of a National Science Foundation 1954 survey presents the following analysis of all forms of support (teaching assistantships, research assistantships, and fellowships from educational institutions, the Federal Government, and other sources):

Field	Per Cent of resident students with stipends	Median stipend
Physical Sciences	58	$1360
Biological (Life) Sciences	62	1395
Psychology	31	1185
Social Sciences	24	1065

The Federal Government, by its present programs of fellowships and research grants, is contributing to this imbalance:

Field	Number receiving Federal support	Per Cent of resident students
Physical Sciences	3399	18
Biological (Life) Sciences	1696	13
Psychology & Social Sciences	512	2

While it is desirable to augment the federal support of graduate training in all the sciences, it is clear that the greatest deficiency is in the social sciences. The 1954 survey disclosed that there were, in the social sciences, excluding psychology, only 15 students with Federal fellowships, and only 95 assistantships on Federally financed research projects. There has been no substantial change in these figures in the subsequent years.

Financial support for university research in behavioral science comes from university funds primarily, and from private foundations and government. The 1954 study conducted by the National Science Foundation reports total support of social science and psychology as 19 million dollars, and of biology as 34 million. Of this total, Federal Government funds constitute 42 per cent in social science and psychology and 53 per cent in biology, as compared with 84 per cent in physical science.

The number of college graduates who are professionally employed in the psychological and social sectors of behavioral science is estimated at 55,000, and in the biological sector, slightly more. Of these, one-third have received graduate education to the Ph.D. degree. Together these constitute two-fifths of the nation's scientists.

More than half of the behavioral scientists are engaged primarily in college teaching. The remainder are employed in government, business, industry, schools, and so forth. The proportion active in research, except in biology, is less than in physical science.

Basic research, as distinguished from applied research and development, is essential for the maintenance and continued growth of any scientific field. The requirements for basic research, and the desirable conditions for fostering and supporting it, were presented in the National Science Foundation 1957 report on basic research. Its recommendations for minimum restrictions on the freedom of the scientist, for long-term program-type grants instead of specific project grants, and for integration of outside supported research with the training of graduate students, are equally appropriate for behavioral as for all science.

The support of behavioral science by universities and foundations is predominantly for basic research. This is not true of Federal support, which is greater for applied research, especially in social science. The following table shows Federal expenditures for 1957, in millions of dollars:

Field	Total basic and applied research	Basic research	Per cent basis
Physical Sciences...	647	143	22
Biological (Life) Sciences.....	281	70	25
Social Sciences.....	35	4	13

Although Federal support for basic social science research comes from several departments and agencies (especially the Department of Defense and the National Institute of Mental Health), the National Science Foundation which is specifically charged with support of basic research, reports grants totalling $382,000 in 1957 and about $600,000 in 1958.

IV. EXAMPLES OF PROGRAM NEEDS FOR BEHAVIORAL SCIENCE

Most of the research in behavioral science is conducted by individuals working alone or associated with a few colleagues or students. It is desirable to maintain the freedom and diversity which characterize such work. At the same time it is desirable to support the mixed strategy of establishing a number of programs or institutes, organized primarily in universities, which carry out broad and continuing researches focused on a defined target area.

In areas where the capabilities or interests of individual scientists do not result in sufficient research to meet the recognized need, it is desirable to foster more organized efforts. Further study needs to be directed to identifying such crucial research problems and supporting institutes or research centers to work on them. A few examples are given:

a. An institute for the formulation of general behavior theory. A carefully selected group of behavioral scientists from different specialties would work together intensively for an extended period to hammer out a provisional theory of behavior,

interrelating the hypotheses and findings from different fields.

People who imagine that they operate without any theory about motivations and behavior are as naive as Molière's character who spoke prose without realizing it. Even the most practical people have their individual theories of human nature and constantly act upon them. The trouble is—and this includes most of the behavioral scientists most of the time—that these theories are incomplete (do not embrace what is scientifically established or highly probable and relevant) and are not explicitly stated or subjected to critical scrutiny.

In recent years groups of behavioral scientists have made useful efforts to pool their tested knowledge and construct an economical, unified theory. These undertakings have been impeded by the press of day-to-day teaching, administrative, and research responsibilities. It will be a great value to detach—on a high priority basis—the best possible people (perhaps twenty) completely from such routine duties, put them in a somewhat isolated location, and give them all necessary secretarial, library, and research assistant facilities.

b. Additional foreign area study centers beyond those that at present exist in American universities. Such centers serve to extend the resources for training in the language and culture of foreign countries, and to provide good recruiting grounds for personnel who would eventually be drawn into government service. These centers also conduct active programs of research in the history and current status of political, economic, and social developments in the area, and in the problems of technological and social change, and may maintain close contact, through conferences, advisory panels and other procedures, with the problems and needs of the governmental agencies concerned with foreign programs, intelligence collection, and policy formation.

c. A program for the study of processes such as concept formation, logical problem solving, thinking, and decision making, including the use of electronic computers to simulate the theoretical models of such functions.

d. A program on the use of social statistics in the development of theories of social change and the prediction of future trends. In the field of economics the work of the National Bureau of Economic Research and of the Brookings Institution has yielded concepts and methods which have become important in Federal economic policy. Although there is not now any systematic effort to conduct comparable research in the social field, the need is acknowledged. The Federal Government now collects some of the needed data and could collect more if there were a solid basis for knowing what would be useful.

e. An institute for the study of the identification, motivation, and education of talented students for useful careers in our society. The most effective use of the nation's brainpower would be supported by research on occupational aptitudes, conditions for fostering creativity, evaluation of special educational programs for the gifted, and so on.

V. TOOLS AND TECHNIQUES FOR BEHAVIORAL SCIENCE

There has been great advance in the tools and techniques used in behavioral science. These include: Microelectrodes that are so small they can penetrate and make electrical recordings of a single cell in the brain and still not disturb its functioning. Computers which can simulate some aspects of behavior, and computers which can speedily analyze social data of a complexity heretofore unapproachable. Radioactive tracers to study function of the nervous system. Game theory—a mathematics of decision making, maximizing profit while minimizing loss. Survey centers with nationwide field staffs. Electrically controlled apparatus enabling animals to choose how much drug or current they will apply to a particular brain center penetrated by a microinstrument. Quantitative means for measuring the content of information in flows of conversation. Electronic filters for separating the frequencies of speech which convey feelings from those which convey ideas. Many of these techniques are costly compared with the methods used in the

study of man by the solitary investigators of a few decades ago.

With such tools behavioral scientists can investigate crucial areas, vital concerns of individuals and society in our nation and the world. In so doing they will attack some problems novel even to those acquainted with the classic status of these fields only a few years ago.

Because the needs of behavioral science for research facilities are different from the more traditional laboratories of the physical sciences and the libraries of the humanistic studies, they have not been so easily recognized and met. But the current developments of experimental and quantitative methods, and the requirement to study social behavior in real-life situations where it occurs, make clear the need for major research installations whose cost is beyond the means of most universities.

While a systematic analysis has not been made of facility requirements, a few examples may be given:

a. Electronic data storage and retrieval mechanism for the Human Relations Area Files, a compendium of categorized information on several hundred societies of the world. The data collection and classification have been continuous over the past 15 to 20 years, and much of this work has been supported by grants from the military departments. The use of new data-processing methods for the hundreds of thousands of items seems worthy of exploration.

b. Electric computer centers, needed for behavioral science research, just as for other areas.

c. Laboratory for research on primate behavior, with facilities for breeding and care of large numbers of animals. The study of monkeys and apes has shown promise for many aspects of behavior (such as thought processes, genetics of behavior, and controlled infant environments) for which other animals are not so suitable.

d. Survey research facilities are essential for many kinds of research. The maintenance of a well-trained national field staff, however, is too expensive for any university to support.

VI. CONTRIBUTIONS OF BEHAVIORAL SCIENCE TO FOSTERING THE PEACE

President Eisenhower has asked for a "science for peace." The issues which can be attacked by behavioral science are the human ones whose solution can guide world affairs along the course from cold war to ultimate peace. These are the most crucial of the many applied problems to which the sciences of man address themselves.

Professor I. I. Rabi recently stated this need clearly and forcibly:

> We have to solve the problem of living together on this planet or we won't live. By we, I mean the major part of the globe, the United States, Russia, and other nations. The end of our national existence is in sight unless we solve the problem.

There has been almost no systematic research in behavioral science concerning international relations and diplomacy, negotiation, the prevention of war, or the operation of arms control systems. Yet our most striking diplomatic successes have been mixtures of technology and politics. Such was our open-skies proposal and our atoms-for-peace plan. Human technologies can also be employed. Behavioral scientists could make a specific contribution in this.

Immediate efforts toward national defense should be paralleled by research to discover methods for achieving more permanent and satisfactory means of international agreement. Such discoveries would provide greater security for peoples generally than the invention of any new weapons system.

This is a mission which demands all the wisdom and imagination that behavioral scientists can bring to bear. It is aimed at operating upon the causes rather than the means of war. This can involve efforts to alter the attitudes which create tensions among nations. It also can include objective studies of what other peoples really think of us; clarification of our own self-image and aspirations; scientific devising of our information programs; development of assistance programs to suit the culture of receiving countries; and reduction of the stress on underdeveloped nations we aid, which results from technological change. All these

very practical isuses in relieving international tensions are vital and researchable, but they are receiving little attention.

VII. THE PLACE OF BEHAVIORAL SCIENCE IN NATIONAL DEFENSE

Though not the application of greatest human hopefulness, certainly the military uses of behavioral science lead all the others so far in the amount of Federal money expended. For example, during World War II, there were many accomplishments. Standardized texts of ability markedly improved the efficient selection of officer candidates. In the Air Corps training failures were reduced from 61 per cent to 36 per cent. The Army's research division developed techniques for screening potential psychiatric casualties among military recruits. Naval psychiatrists were able to improve the rate at which such casualties were returned to duty. At the beginning of the war, only 5 to 10 per cent of psychiatric casualties could be returned to duty, but with the use of new methods, psychiatrists could salvage 60 per cent for front line duties in 2 to 5 days and an additional 30 per cent for service in rear areas. Some problems were even demonstrated to be nonexistent. For example, there was much talk about the causes of absenteeism among workers in war industries. A survey established the fact that there was little or no absenteeism not attributable to such normal causes as sickness, women having to stay home to take care of children, and so forth. More effective methods of convincing Americans to buy war bonds were inaugurated. Certain testing devices used in selecting American troops were later found to correlate positively with aspects of their combat performance. Engineering psychologists aided in the design of weapons and equipment. Anthropologists successfully predicted the morale of the Japanese population. The list could be extended at length.

Each of the areas of basic investigation mentioned earlier in this report has military implications, and as the methods of war grow more efficient the need increases for more precision in understanding behavior of men in military systems. Selection of the right personnel; training; management and leadership; and the maintenance of cooperation and morale—all these are everyday problems which become more acute in military life. Brainwashing; psychological warfare and countermeasures; obtaining, processing and evaluating of intelligence—these are problems of special importance to the armed forces. The functioning of man, with his limitations, in planes moving faster than sound or in electronic detection systems supplying information at great speeds; effects on him of low oxygen, extreme cold, high gravity, weightlessness, cosmic radiation; the emotional stability of men on long missions in isolation, or under great stress of decision responsibility—all these are problems for behavioral research. They can arise in other walks of life but are particularly pressing in the military situation of new weapons, new strategies, and new possible battlefields in space.

As weapons develop from jet planes to space ships, from atomic cannon to ICBM's, from independent fighters to intercontinental electronic warning systems, the demands on man will usually increase. Gross variability in human performance once was acceptable in war, because so many other factors fluctuated. But in modern weapon systems the greatest source of error is man, and consequently a precise science of his acts is urgently required. The newness of our defense problems requires imaginative reexamination of old assumptions and non-traditional research on the human factors in war.

VIII. CONTRIBUTIONS OF BEHAVIORAL SCIENCE TO THE NATIONAL STRENGTH AND SPIRIT

The strength of a nation depends on its technical and material assets, and on the scientific research which constantly expands these physical resources. But national strength is equally dependent upon human factors which determine how effectively physical resources are used: the health, morale, and motivation of the population, as well as the formal and informal organization of the society. The well-being and happiness of its citizens are goals of a democratic society, rather than mere means for the attainment of greater material

strength. And the productivity of society is dependent upon all these human factors, which are the subject of study of behavioral science.

Americans are still only dimly aware of the central role our educational system plays in our present way of life and in our future development. Fortunately President Eisenhower's "safety through strength" program stresses education as one of its main points. His recommendations on education include the selection and guidance of the gifted and the evaluation of methods by which they are trained. These tasks demand the collaborative efforts of behavioral scientists and educators.

The population of the United States will increase and automatized technology will expand so that in a few years there will be enough manpower, but not enough highly trained manpower. This situation can be expected to continue for the next 10 to 20 years. The qualitative shortage will come from failure to provide scientific and technical education in our public schools rather than from scarcity of pupils with capacity for such education.

Future developments may force Americans to undergo significant changes in attitudes toward: expenditures for national defense; alertness to the possibility of major emergencies; resistance to enemy psychological warfare; and acceptance of the necessity of a professional military force. These will be achieved only through modification of long-established traditional attitudes, a process calling for careful study in behavioral science. Again, in the event of war, one of the gravest dangers is loss of morale, despair or actual panic. These might be brought about by sudden crisis, or by a slower deterioration of military position coming, nevertheless, too rapidly for the people to adjust.

There might be loss of faith in our own strength or capacity to resist attack or alarm over radiation from fallout or astonishment at an unforeseen collapse of the economy. Additional research in behavioral science is needed to enable us better to forecast such reactions and to mitigate their consequences.

Another rapidly developing field is that of human relations in labor and industry, which can increase the general productivity of the country. It is important to support needed basic research in this area, which probably cannot or will not be directly supported by industry or labor.

In many ways the primary internal social and medical problem of America is mental disease, broadly defined. We have 750,000 people shut away from productive existence. There is a much larger number unable to make their expected contributions to society or even to live happily with themselves. Nor can they live happily with others; witness such problems as divorce and juvenile deliquency, racial and ethnic strife.

There is a special role, also, that behavioral science can play in all research. In science education, selection of scientists, organization of research groups, and encouragement of research creativity, the problem is fundamentally one of behavioral science rather than natural science, even though it may concern the development of engineers, mathematicians and physicists.

We have catalogued in the preceding paragraphs a few areas in which a democracy has a natural interest in behavioral science. Fear of the Russians, and measures we must take to meet the Soviet challenge, are not the only motives for supporting such research. Preservation of our advantages and sharing them with the world are other motives even more compelling than fear of a strong enemy. Whatever the motive, the conclusion is the same: the United States must be strong physically and morally, and therefore needs to pursue such investigations in behavioral science as will contribute to this strength.

Reality and unreality are not absolute properties of certain events; rather, any experience is real or unreal depending upon its conceptual index and libidinal significance. Reality testing gives meaning to experience. Reality sense provides for the intensity of experience. Reality value means that a man so believes in a certain reality that not only does he allocate it in a wider symbolic system and make predictions about it, but he is willing to take action upon it.

REALITY SENSE AND REALITY TESTING

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ONE of life's strange paradoxes is our willingness to believe in something without any evidence to support the belief. We may even adhere to a theory, and maintain it, long after it has been disproved. Most of the accepted notions that motivate us are largely clouded in irrational conjecture. Logical concepts based on observed evidence do not, in themselves, carry conviction, and the most ardently defended views of life are often without foundation in fact.

Why are we convinced of some things and not of others? What is the origin of belief? How does it differ from knowledge?

Ego psychology has made considerable progress in the past few decades, but many problems are still only spoken of without actually being explained. One of the most flagrant examples of this is *reality testing*. The means by which we recognize reality and act upon it are almost as unknown as is the intuitive faculty which endows certain experiences with the conviction, "This is real!" Reality testing has often been taken for granted, as though it consisted of a single process. Analogously, reality has often been treated as an unambiguous fact of existence, fully developed, and the common property of mankind.

The purpose of this paper is to clarify the processes of *reality testing*, *reality sense*, and the *concept of reality*. The thesis is that reality sense provides the libidinal coordinates, while reality testing supplies the intellectual coordinates of experience. The concept of reality emerges from the socially accepted meaning of common experience. In other words, reality is not an absolute property of the world, but is a product of reality evaluation, a complex process of thought and feeling which decides what is real as much by the intensity as by the invariant nature of experience. The concept of reality in every age is a reflection of man's psychic processes—sometimes intellect predominates and sometimes emotion—but neither operates without the other. In our estimation of reality we are inescapably both Stoics and Epicureans.

REVIEW OF THE LITERATURE

Psychological studies of perception, concept formation, learning, reasoning, and so forth, actually are efforts to investigate the details of the processes by which man evaluates and adapts to reality. In this section, only psychoanalytic studies of the reality experience will be presented, plus a few psychological and philosophical works which have special significance for the point of view expressed in this paper.

Throughout Freud's works there are many clinical and theoretical references to reality and reality testing. Although not explicitly stated by Freud, his observations and conclusions may be summarized under four separate headings, each representing a different approach to the problem. Most of his followers have investigated one approach or

another. These four are: the reality principle, the development of object relationships, aberrations in reality perception, and the "oceanic" feeling, especially as it relates to religion and art.

The distinction between the reality principle and the pleasure principle is one of Freud's most fundamental contributions. Reality, to Freud, is something apart from the percipient, an external object which the mind handles; it does not contribute to its structure. Although Freud distinguished between psychic and physical reality, the "reality" of the reality principle is that which imposes an external restriction on the pleasure seeking ego (22). In the course of development, reality interacts with instinctual processes to permit the development of several important mental functions. Thought processes, based on memory traces of early gratification, consciousness, attention, memory, preceptions, judgments, and ideas, all emerge from the limitation of free libidinal expression. The latency period is forced upon the ego by reality, but the pleasure principle continues to operate through phantasies and dreams. In fact, the reality ego, which always seeks the useful and avoids the harmful, preserves the pleasure ego by enabling it to find alternative means of fulfillment in education, thought, art, and especially in certain neuroses. Reality testing, or the intellectual, secondary processes by which experience is judged, impulses are modified, and activity is guided, develops on the basis of frustrated libidinal strivings. In contrast, the primary process disregards reality and reality testing. Thought, images, memories, phantasies, and psychic and physical reality are treated impartially and without distinction in the timeless, chaotic domain of the unconscious.

Freud believed that the development of object relations starts with the relationship between the mother and the child. Primary narcissism finds the child concerned solely with objectless gratification. With the perception of the mother as the source of both fulfillment and frustration, the polarity of subject and object, pleasure and pain, good and bad, is established. What is bad, alien, or external has identical meaning to the child, according to Freud (27), inasmuch as he indiscriminately deals with reality by either crude introjection or total expulsion. In the course of maturation, recognition of the difference between internal and external reality develops, then between memories and current perceptions. Indeed, it is the aim of reality testing to unravel the distortion between the subjective image and the objective fact. Both the pleasure principle and the reality principle describe attempts to recover, within each new experience, the image of the original object which provided unlimited satisfaction. Reality testing, for Freud, has biological motivation, and hence is itself grounded in the instinctual life. Thinking, to Freud, is a trial action, prompted by libidinal frustration. It ceases when we are willing to act upon an image or object. At this moment, reality itself emerges.

In the neuroses (24), reality is avoided. Instead, a substitute for reality is attempted through phantasies. In the psychoses, reality is remodeled, rather than merely being avoided. A new reality is constructed on the basis of archaic images. Daydreams of normals and neurotics, the play of children, and the imaginative creations of artists all arise from the vast reservoir of unfulfilled instincts and infantile recollections. They are efforts to modify reality to bring about pleasurable experience (25). This is related to the "oceanic" feeling (21). Freud denied that this sense of the limitless extension of the ego and the undifferentiated unity of the self and the universe was the source of religious conviction. To Freud, the "oceanic" feeling was an expression of primordial narcissism, which itself is the source of the reality principle. Both Sachs (59) and Wittels (71) developed the theme of how the elaboration of early phantasies contributes to the sense of the ego and to artistic creation. Early libidinal positions are never lost; Freud proposed that they are like ancient civilizations which are supplanted but never completely destroyed. How meaningful this view was to Freud is illustrated in an episode of unreality feeling which Freud experienced while visiting the Acropolis (36). He analyzed this as being connected with

a forbidden wish to surpass his father and with incredulity that he would ever visit such a wonderful place. The inescapable reality of early memories and ancient cathexes which may abruptly impose itself on later perceptions became a cornerstone of psychoanalysis. In other clinical studies, Freud demonstrated how an early memory of parental intercourse could cloud the perception of later reality (23). His studies on fetishism (26) indicated that, despite the fact that one reality may be a denial of another, both may persist side by side.

Nunberg (49, 50) regards reality testing as an ego function, distinct from the id, which knows only inner processes. It is the nature of reality testing to doubt the existence of the world, and to delay recognition or denial of a perception. Although Nunberg recognizes that what is real at one time may not be real at another time, he maintains a fixed distinction between an internal (autoplastic) and external (alloplastic) reality. Perception, thought, and action are the major functions of the ego, but it is action which is the basis for reality testing. If an object can be acted upon, reached out for, and touched, it belongs to external reality. However, when reality testing falters, then unconscious ideas may be perceived as actual images. Nunberg calls this "identity of perception." Working through the repetition compulsion, the archaic superego sanctions the acceptance of these unconscious ideas as reality. In depersonalization, current cathexes are abruptly lost. Secondary narcissism is damaged and, as a result, reality testing is impaired. Study of the details of Nunberg's viewpoint show that reality testing is a more complicated process than the simple tactile method by which he claims to distinguish the real from the unreal.

Fenichel's (14) conclusions were similar to Freud's with respect to reality testing. The task of reality testing, he asserts, is to distinguish between an object and a mental image. This is accomplished by action, particularly the symbolic action involved in speech and thought. In depersonalization, libido is suddenly withdrawn from perceptions of the world to the mental representations of the body image.

In her study of the adaptation to reality by young infants, Benedek (4) proposed that perception of the environment and differential responses to stimuli follow instinctual gratification, rather than frustration, as Freud had emphasized. By learning to wait for mother in a state of confident expectancy, primary object love develops. Fulfillment and the formation of dependable object relations determine the ego's capacity to master the object world. Anxiety develops when object relations are disturbed through frustration. French (18, 19) follows this point of view, asserting that anxiety of this type only inhibits the learning process, and, along with the persistent fear of trauma, interferes with the adaptation to reality. Primary object love, actively sought and fulfilled, is the prototype of both mastery and reality. Reality testing corrects the distortions of perception in which past and present images are confused. The ego learns from mistakes and does not repeat them. French stressed the fact that the archaic superego imposes the past on the present through unrestricted operation of the repetition compulsion. Waelder, (66) too, recognizes how the superego participates in reality testing, although he notes how id components also share in this process. Reality testing suffers when only love or hate is felt; a constant self-appraisal is necessary if man is to be free to realize his potentialities, a dimension beyond mere adaptation to every day events. An enlightened superego, Waelder insists, is an indispensable ingredient of reality testing.

Balint (1, 2) maintained that the frustration of object love is a more primitive response than is primary narcissism. In later life, frustration may be accompanied by such strong emotion that the distinction between shared and unshared perceptions may be lost. Reality testing may be overwhelmed by the hypercathexes of certain ideas. There is no absolute boundary between the ego and reality, according to Loewald (45). Rather than to adapt to the stresses imposed by an external reality, the ego defends itself against the loss of reality by seeking integration with the world as it existed with respect to the primary love object, mother. The concept of reality as a hostile, opposing force is derived from the primitive conception of the father who interrupts the original unity between

the mother and the infant. How the infant is able to recognize the intruder is not clear, however, inasmuch as a high degree of reality testing is presupposed for this to occur. All of these authors share the view that reality testing consists in re-establishment of an infantile state of fulfillment, and that certain frustrating experiences only perpetuate the need to find a synthesis between present-day perceptions and bygone images. According to Ferenczi (15, 16), the reality principle emerges from a series of renunciations which the pleasure principle inevitably undergoes in the course of maturation. He believed that for every stage at which these renunciations occurred there is a corresponding neurosis, with a different "sense of reality" for each. Even though narcissism is gradually surrendered, both ambivalence and love serve as protective devices to insure a measure of gratification, he thought. This view also entails the basic psychoanalytic principle that there are specific object relations which appear at various stages in development. Laforgue (39, 40) more recently advocated a hypothesis similar to that of Ferenczi. Not only does inhibition on various libidinal levels have a selective effect on the type of neurosis which develops, but, he states, there is a corresponding type of reality which comes about through affective necessity. It is clear that however readily various psychoanalysts have accepted the common sense, mechanistic view that there is a fundamental distinction between subjective states and perceptual objects, the actual findings of psychoanalysis demonstrate that the ego does not merely adapt to the world, but that different modes of thinking contribute to the development of different kinds of reality. Zilboorg (73) described this clearly by pointing out that the sense of reality is a fluid quality of the psychic apparatus by which man masters and modifies the concrete. The instinctual elements which promote psychological reality become an integral part of every new object representation. In the act of adjustment to reality itself, the reality sense of the patient's psyche guides his perception.

The earlier psychoanalytic writings emphasized the libidinal aspects of reality testing, and did not explicitly distinguish between the reality principle, reality sense, and reality testing. With the development of ego psychology, more attention has been devoted to the cognitive aspects of reality testing. Piaget (53), for example, investigated the maturational process by which egocentricism in young children is gradually changed into a conception of a universe of cause, time, space, and substance. He postulated two formative processes of assimilation and accommodation which are antagonistic in infancy, but which finally become coordinated. Early sensorimotor intelligence, Piaget states, becomes conceptual thought when, through internalization of progressively differentiated behavior, representation takes the place of immediate action. Concomitantly, practical goals of action are replaced by abstract norms of truth whch lead to socialization and objectivity, rather than to mere self-seeking satisfaction. The concept of an object, according to Piaget, does not develop until the capacity for representative, or class, thought develops. It is only when objects are conceived as a social phenomenon that reality and a sense of permanence begin.

Hartmann (30) and his associates, Kris and Loewenstein (32, 33), have applied psychoanalytic concepts to the cognitive elements of reality testing. It is the ego which tests reality, Hartmann proposes, and this is accomplished through all of the ego functions. The ego adapts to reality and masters it largely through its inhibitory and synthetic functions, which are based on the processes of thinking, perception, and action. Hartmann recognized that *the reality principle cannot be derived from the pleasure principle alone*, as many of his predecessors had maintained. Drives, in themselves, do not guarantee adaptation, and inhibition alone is not enough for the development of reality testing. Both synthetic and differential ego functions are required. These operate with neutralized energy within the autonomous ego field. Hartmann's view is that both libido and aggression contribute to the driving power of mental life. In the course of maturation, the ego withdraws cathexis from objects. This neutralized energy becomes countercathexis and is responsible for main-

taining the distinction between the self and the world.

Schachtel (62) has recently propounded the concept of "focal attention" in an attempt to provide further cognitive clarification of reality testing. He defines focal attention as the capacity to center attention on an object and regard it from many sides. It is a directional, secondary process which leads, Schachtel states, to differentiation of the self from objects, and of both from mere images. However appealing may be the concept of focal attention as an agency of reality testing through selective perception, it appears to be a result of reality testing, rather than a primary function.

Hart (29) stressed the importance of logic and reasoning in reality testing, but emphasized that adaptation involves action, and not just thought. He distinguised between the tests of reality and the mastery of reality, a distinction more easily hypothesized than maintained in practice.

The gradual change in psychological views about reality testing has indicated that the reality principle is a complex expression of many ego functions, rather than being merely the residue of frustrated libidinal strivings. The three primary ego functions, namely, perception, thought, and action, cannot exist apart from each other, and so are coordinated in practice. Perception is not only a basis for action, but is inseparable from it, as stimulus is inseparable from response. Action without perception is blind; action without thought is aimless. Thought without action is futile; thought without perception is dead.

Although interest in the reality principle and the genesis of object relationships has tended to dominate psychoanalytic studies of reality, some workers have followed the other two directions which Freud indicated, namely, the aberrations of reality perception and the sense of reality. Wittels (71) regarded the latter as a "neglected boundary" of psychoanalysis. He saw manifestations of "erlebnis" in the "oceanic" feeling, in the experience of anxiety, in the difference between the sexes, and in the sense of the ego itself. Sachs (59) found that the sense of reality emanating from the artist's unconscious gained expression in his creations. In 1930, Malamud (46) distinguished between the sense of reality as an emotional experience and reality testing as a function of the ego. He found some disturbance of the sense of reality during the course of most mental illnesses. A conflict between emotional desires and conventional standards may be settled by an alteration in the flow of libido. Conscious perception, Malamud proposed, is a resultant of two vectors, subjective values and objective stimuli, so that reality is autoplastic as well as alloplastic, rather than being something wholly external. He drew an analogy between repression and reality testing by showing that both processes hold the emotional factors of perception in abeyance, a hypothesis which reflects Freud's concept of negation (27), Schachtel's "focal attention" (62), as well as recent views of Linn (44), Kamm (37), and Frumkes (28).

By integrating the discursive writings of Federn (13), Weiss (69, 70) has attempted to rescue the sense of reality from the penumbra of psychoanalysis and has brought it into functional relationship with the ego. He noted that more than mere sense perception is required for the feeling of reality, because intact sensation may exist along with severe states of unreality. According to Weiss, all sensory stimuli, whether derived from external or internal sources, convey a sense of being real only as they enter the ego boundary. Only those elements of experience which are "egotized" are perceived as part of the "self," and this occurs at the ego boundary which separates the self from the non-self. Ego boundaries vary, however, and so, perforce, does the sense of reality for various objects. Although the different sense organs are regarded as the "windows" of the ego boundary, reality sense depends upon the libidinal cathexes of each experience, rather than upon sense perception alone. Reality testing, thus, is a function of ego cathexis, and it is by means of countercathexis that the ego prevents inner stimuli from impinging on current perceptions. Mental contents are felt as unreal when they are divested of medial libido, the source of primary narcissism. When this happens, depersonalization may occur, for the decathected body image

is then treated as "real" only in the sense of being an object outside the ego boundary. Weiss noted that in severe states of depersonalization only the eyes and mouth may be felt as real. Correspondingly, Bender (3) found that in organic brain disease the most reliable perceptions were in the facial area, while other parts of the body developed severe perceptive disorders.

Lewin (41) investigated the illusory sense of reality which occurs in certain psychiatric disorders. He believes that it represents a temporary revival, through recollection, of early feeding satisfaction. The archaic memories are renewed and are treated as though they were current perceptions. The infant's mouth, Lewin states, is both the source of pleasure ego and the point of initial contact with reality. At first, pleasure values and reality values are the same. Lewin distinguishes between three worlds of experience, each with its own sense of reality: the oral world, the world of perpetual flux, and the world of words and concepts. Reality sense, however, originates in the nursing experience of the oral world, so that in certain regressed states the sense of reality which is revived is one appropriate to the early oral world. He contrasts the reality sense of direct experience with the emotionless reality sense which is attached to what is learned from others. Intellectual reality, which belongs to the domain of detached concepts, completed weaning, and attenuated satisfaction, is farthest removed from the intensely personal, immediate, oral reality.

The concept of the psychological field, as developed by Kurt Lewin (42, 43) has clarified certain aspects of the reality experience, specifically as it reflects analogies with the theory of the libidinal and conceptual fields. Reality testing, in common with all psychological functions, involves manifold social fields of force which oppose and modify the actions of an individual within that field. According to Lewin, there are degrees of reality with respect to different dimensions of psychic life, and what is real in one sphere may be unreal in another. For example, magical ideas, such as superstitious beliefs, may be quite convincing, despite the lack of objective verification in physical reality. Moreover, Lewin asserts, processes which have powerful needs coupled with powerful restrictions to expression usually have a greater degree of reality. All psychic processes consist of multidimensional valences arising from "tense systems" within the individual. These valences release their energy through the forces which operate within the field. Such dynamic processes contribute not only to the appreciation of reality, but also to the structure of reality.

It may now be seen that psychological investigators in general have concluded that the ultimate source and substance of reality testing is discovered in man's experience of himself, rather than in a purely mechanistic separation of subject and object. Whether the responsibility for reality testing is placed on the libidinal urges or on the autonomous ego, on sense perceptions or on archaic images, in the social field or in the satisfactions and frustrations of mother-child relationships, reality is rooted in the self, which sees it in its own image, regardless of how this is determined. The common nucleus of both the ego and its total field is the body image, which represents the condensed and fused experiences of many origins and many modalities. For Schilder (63), the body image is a dynamic, ever changing, always incomplete residue of a multitude of centrifugal and centripetal experiences. Although derived from many physiological sources and sensory schemata, the postural model is appreciated as a unit. Community concepts, through appersonization and projection, contribute sociological factors to the body image, and thus modify the individual's self-conception. The libidinal factors of the body image determine the relative value of the various body parts, as well as the means by which the body parts of other people are brought within each person's self-image. In this way, Schilder attempted the formidable, perhaps impossible task of integrating physiological, libidinal, and sociological forces within a total field, with the body image as the nidus of this field.

The various psychological views of reality are based on long familiar philosophical hypotheses (17). The belief in an absolute distinction between subject and object, be-

tween psychic and physical reality, between mind and body for that matter, is a part of a philosophy of materialism and formism (52). The present-day emphasis on field theory and relativity, on process and operation, is based on the philosophy of organism and contextualism. From the latter viewpoint, there is no absolute distinction between reality and unreality, between thoughts and things, between the "self" and the world. Indeed, such concepts are meaningless unless they are defined by reference to a specific logical system of constructs. Today, as in all ages, our conception of the ego is projected as a model of reality. Reality, like the ego, is not static and absolute, but is viewed as a dynamic, conditional system, at once logical and emotional.

In 1869, William James (34) described the reality experience in a way which has not yet been surpassed. Only by the use of psychoanalytic concepts has it been possible to extend the scope of what James perceived. There is no single, abstract "reality" opposed to an even more abstract "unreality", according to James. The world contains many realities. Things are unreal only when they are contradicted by some other thing in which we believe. The source of all reality is in ourselves. Whatever we turn to with a will, because it commands our attention, has a higher degree of reality. "A whole system may be real," James said, "if it only hang to our ego by one IMMEDIATELY STINGING TERM" (34, p. 297). There are many orders of reality, but the ultimate of ultimates for our belief, or sense of reality, is that sense of our own life which we possess at every moment. It is this which accounts for the pungent reality with which our own senses are endowed. "The whole distinction of real and unreal, the whole psychology of belief, disbelief and doubt, is thus grounded on two mental facts—first, that we are liable to think differently of the same (thing); and second, that when we have done so, we can choose which way of thinking to adhere to and which to disregard" (34, p. 290).

Reality testing, according to James, is based on the requirement that conceptions must terminate in the world of orderly, sensible experience in order to prevail. Thus, the reality of an object depends not only on its emotional ties, but upon verification. In order to compel lasting belief, the processes that we call reality sense and reality testing must fuse in vivid, uncontradicted sensory experience. Specifically, sensations which arouse pleasure and pain, and which incite action, determine the reality of objects. Conceptual systems, too, must include sensible objects in order to be accepted as true. Finally, that theory will be believed which, besides offering us objects able to account satisfactorily for our sensible experience, also offers those which are the most interesting, and which have the most appeal to aesthetic, moral, and active needs. It is evident that modern psychological theories of reality which stress the emotional and intellectual aspects of its structure and the integration of thought, perception, instinct, and action for its discovery and testing are based on the propadeutic of William James.

In his philosophy of relativity, Ernst Cassirer (8) proposed that reality consists solely in the logical differentiation of the contents of experience and their arrangement in an ordered system of dependencies. We deal with "objectivity," rather than objects, and this does not depend so much on the vividness of impression as it does upon the wealth of inner relations of an experience and the number of consequences to which it leads. It is not what an experience is, but what it is *worth* in the structure of the whole that is important. Experience is objective when we can understand how each element is woven into the whole. The concept of "thing" is simply a metaphorical expression of the permanent connection of phenomena according to law. In this way, it is the conceptual correlation of systems of experience which makes up reality. Empirical knowledge seeks ultimate invariants and fixed connections between events; this is the meaning of "objective." In turn, all that is impermanent, in terms of concepts or logical connections, is "subjective." According to Cassirer, the distinction between "subjective" and "objective" is only relative, for the elements of experience change or persist only with reference to another element,

which first must claim constancy for itself. In this way, the process of experience and its object are correlated, rather than opposed, for total experience requires both a unifying sensory experience and an act of conceptual completion on the basis of a logical principle. So conceived, reality may be divided into conceptual centers about which various continuous phenomena are ordered. Each center belongs to partial fields, which are distinguished by their systematic meaning. By inference from Cassirer's thesis, reality testing consists of a constant act of self-correction by the experiencing ego in order to maintain the "objectivity" of successive events. However, what is "subjective" in reality testing is also a product of intellectual analysis, and must consist of the material which the continuous process of adjustment and correction has excluded. In a corresponding manner, reality sense, or the feeling of conviction, applies to those judgments which claim unconditional validity for their contents, the elements of which seem to have only hypothetical significance. In reality, whole events are always dealt with, rather than with their abstract constituents. Instead of judging an object as real, according to Cassirer, we assert our logical orientation with respect to an experience.

Whitehead (35) provided another view of reality, and dealt chiefly with the ways in which the organism itself contributes to the structure of the universe. Intuition, or the direct, immediate experience of fundamental data, according to Whitehead, is both the source and the test of all knowledge. Intuited data, although concrete, subjective, irreducible, and unproved, have objective value as "actual entities." Whitehead viewed reality as an infinity of actual entities which grow, by reason of "feeling-vectors," called "prehensions," toward a state of satisfaction, or toward "concrescence," which is the emergence of other new, actual entities. Each entity achieves reality by growing, while its own evolving and extending feelings participate in both the formation of its world and of itself. For, although actual entities may perish subjectively, Whitehead believed that they are objectively immortal as "eternal objects." The discovery of reality is actually a matter of self-realization, for there are, to Whitehead, three tests for the validity of perceptions: their force or vivacity, discovery of the origin of the experienced data in referents, and the degree to which the future conforms to expectations. Reality is thus at once a creative emotional experience, an element in a conceptual field, and an emergent phenomenon.

The profusion of theoretical subtleties which abound in any systematic philosophical endeavor makes it urgent to separate the plausible from the actual. The force or vivacity of a perception, which both James and Whitehead present as irreducible tests of reality, is intellectually defined by Cassirer in terms of the inner logical relations which an experience possesses. In some respect this points up the contrast between the "poetic" view of life and the "scientific" view. At one extreme is the religious existentialism of Kierkegaard (7) who held that the only solid basis for our relations with the universe is through the subjective elements of experience. According to him, reality may be found only in uncertainty and despair; the irrational, comic, and absurd lead to the highest truth. The subjective is both reality and truth; and this is determined by the passion with which an object is held. At the other extreme is the philosophy of operationalism (54) which emphasizes definition and prediction of observed phenomena as the basis of meaning. Reality tests, from the operational point of view, consist of verification procedures by which invariant elements of experience are repetitiously discovered. In the psychological domain, it is what is done with a concept that defines its meaning, not just what is said about it. The ego, for example, is defined by its functions, what it does, and what can be predicted about it, rather than by abstract qualities which can neither be operationally defined nor tested. Although the operational viewpoint cannot explain reality sense as readily as it establishes the conditions for reality testing, it rightly demands that reality sense, as a theoretical construct, must satisfy certain criteria before claiming recognition as a useful scientific concept.

However distant in time or temperament, various philosphers have shared the view that, in the end, truth and reality are but two correlative aspects of a total experience (17). Reality is an emergent feature of experience; the *standpoint* from which the cavalcade of experience is viewed is the invariant, not the experience itself. Reality testing is based on some form of action, in accordance with ordering principles which are themselves accepted by unequivocal intuitions. Reality sense, in so far as these philosophers acknowledge it, is an expression of a total feeling state, not too far removed from Freud's primary narcissism or Federn's medial libido, which reaches out to influence other structures, and, in so doing, changes itself. The old distinction between appearance and reality cannot be maintained. In a completely adequate theory, Pepper (52) claims, everything is real; unreality is merely the name for the fact of inadequate interpretation on the basis of hypotheses which claim, but do not possess, world wide scope. For what is real in one philosophy may be unreal in another.

REALITY SENSE

Human experience is influenced by a variety of personal beliefs and attitudes. Some of these are explicit, but there are many more which are only inconspicuously present. In its never ceasing self-corrective efforts, the ego adapts to the stresses and strains of the world by finding more significance in certain beliefs than in others. The wish to find constancy in the flux of experience, to be certain and resolute instead of doubtful and indecisive, to proceed confidently instead of groping timidly, is based on a sense of conviction that some experiences, whether in the form of ideas, emotions, or attitudes, are more trustworthy than others—that some things are real, while others are not.

The way in which we differentiate between an atom and a memory, a bird and an equation, or a depressed mood and a chair, involves complicated intellectual activities. Each of these events, however, as mere experience, conveys a sense of conviction or belief independent of the full knowledge of its inner structure. This is *reality sense*. The sense of reality acts on the whole experience, rather than on any of its parts. It is immediate, forcing us to believe beyond dispute. The reality sense is more apparent in the fact of an intense emotional experience than in the rigors of logical thinking, and requires no more proof than do artistic creations or daydreams. In a similar fashion, that which imparts a feeling of being right, the sense of conviction or unchallenged belief, depends upon the reality sense.

When we reflect upon the numerous articles of belief that each of us holds, the vast body of truths, untruths, and half-truths that determines behavior, attitudes, and judgments, how many are the result of careful reasoning, cautious experiment, or recourse to trustworthy sources? Indeed, many of our most pleasing and rational proliferations have sprouted from irrational roots. We are more readily convinced, more easily swayed, by the emotional context of ideas than we are by the truth of what is proposed. Long after the parent's fallibility has been established, his authority may persist. In a depressed person, fancied guilt continues despite his full conscious recognition of innocence. Compulsive rituals are often attempts to undo that which has occurred only in phantasies, which the unfortunate patient finds appallingly real, beyond all dispute or reason.

We believe only what we *feel* to be true. Emotional acceptance is more convincing than reason or reliable information. In everyday life facts always yield to feelings, because the subjective state of a person colors his perception of the entire world. To a masochistic personality the only reality is pain. The depressed patient sees only black, forbidding images. In a similar way, the unique emotional context to which the creative artist refers his experience may elevate the banal or ordinary into realms of universal truth.

Logicians have yet to determine what makes certain proofs convincing, rather than merely reasonable, admissible, or adjunctively true (55). Actually, this is a psychological rather than a logical issue. The components of proof—verified propositions,

accepted operations, and consistent notation—carry their own conviction, and these must be transmitted to the proof itself in order for us to believe and thus act. This does not mean that there is no reality sense in highly abstract intellectual experience. It merely emphasizes that there is a difference between truth and conviction, validity and belief, and that a logical system of theorems and demonstrations is not as convincing as a colorful illustration which uses an already familiar sense of reality.

Unless the emotional component of reality sense is concomitantly present, the most intricate and painstaking reality tests may endow an experience with only a high degree of probability as to the truth of the propositions about it. There are instances where the reality sense puts some experiences beyond the usual methods of reality testing. Indeed, the sense of reality of the self, that introspective experiences are consistent and continuous, is difficult to state in intellectual terms. Pain and anxiety in another person cannot be appreciated, tested, or proved, except by inference from known stimuli and standard responses, or by identification through introspective data. The sense of reality is private; only the description or meaning of certain experiences may be shared.

Acceptances

The operation of reality sense is clearly revealed in our personal articles of faith. Through faith we assert and act upon matters beyond experience. This applies not only to religion, but to any operation or object which transcends personal experience. (This corresponds somewhat to Santayana's concept of "animal faith" (60), but functions mainly in the realm of ideas.) The fabric of belief, which provides a basis for action, assertion, and judgment, is woven with certain constant concepts and ideas. These basic concepts may be named in different ways: presuppositions, biases, primitive terms, self-evident truths, aphorisms, and so forth. In the sense of not being deducible from more primitive ideas, they are irrational. Because they are convincing, believed, and acted upon without serious recourse to alternative notions, they are designated as *acceptances* (61).

The reality sense functions most clearly where acceptances are concerned, for these primitive ideas are essentially irreducible except in terms of other acceptances. Psychoanalysis explicitly deals with acceptances in the form of unconscious attitudes which the ego reflects in response to stress. Thought and language attempt to arrange these beliefs and attitudes into conceptual and symbolic systems. Acceptances, irrational and irreducible, are end points of interpretation, and so provide the basis upon which the ordering principles of mental life may proceed.

Acceptances are ideas which are believed without question. They are not necessarily orderly or well defined; in this form they are called "axioms." Nor are they merely unconscious ideas which come to light only by way of dreams, psychosis, or psychoanalysis. They are beliefs which appeal to the individual emotional predisposition, and are not affected by someone else's reason or information. From a certain point of view, prejudices may be called acceptances, even though the person holding these beliefs rarely identifies them as such. Acceptances appear in many forms and vary from society to society (5), group to group, person to person (56) and even for the same person at different times. Some acceptances are more widely maintained than others, and may appear in any field of human endeavor in which people make judgments about experience. At one time, Aristotle's logic was thought to express the "laws of thought," and Euclidean geometry was considered to describe "reality." Today it is recognized that the former is only an elementary form of a deductive system, while the latter is merely one type of hypothesis. There are many acceptances relating to social institutions which are modified only with difficulty. The divine right of kings was once acknowledged with an unquestioned sense of reality, in much the same way that certain democratic ideals—such as government by representation—are accepted at the present time. The family as the basis of moral behavior, the dignity of man and the immortality of

the soul, the best government is that which governs least, the pursuit of justice for all, the respect for age and wisdom—all of these are acceptances, however worthy they might be and however unthinkable their alternatives. The essential feature of acceptances is their sense of reality for those who hold them, to the extent that any statement about them is held to be true. The emotional roots of personal acceptances are readily exposed by responses varying from rage to uneasiness when someone else seeks even to hold them up to scrutiny, let alone doubt. Such investigation never elicits dispassionate curiosity. No book, article, or discursive exercise is free of the appeal to the reader's acceptances. Even the most rigorous and carefully defined statements make assertion after assertion, the validity of which is beyond the scope of the argument. These unchallenged notions acquire increased conviction with the addition, through information, logic, and reason, of the reality of other experiences.

The analytic, or rational task of ordering experience is slanted from the beginning, according to each person's emotional fashion, or "style." Inevitably it begins with synthetic acceptances which reach into realms of belief for a sense of reality. Our own system of acceptances forces us to select from the catalogue of experience only those items which further establish that which we already believe. Most of us seek certainty, not truth. Thus tension, which is the result of the discrepancy between fact and fancy, is minimized. In everyday life, that which fails to correspond to accepted canons is overlooked, dismissed, or attacked, depending upon the urgency with which the alien idea is forced upon us. Even our faith in the future, that there will be a tomorrow, that there are things which may be sought and found, seen and felt, belongs to the realm of acceptances. In this way, they quietly decree the course of the future.

Experience and intuition

However intense may be the reality sense which is attached to acceptances, it fades when routinely applied to new experiences. The fresh sense of conviction degenerates into dogmatism unless new events are experienced with enthusiasm and with modification of old responses. This implies that mental life, like the physical body, is constantly being replaced and revived, that old ideas and acceptances lose and gain cathexes during the phases of growth and development. In the course of progressive assimilation of new experiences into the emotional field, the reality sense must extend to ever expanding and differentiating perceptions.

The reality of objects is recognized through the process of *intuition*. Out of a variety of perceptions, whether of sensory objects or mental contents, the whole suddenly emerges, distinctively and indisputably. Intuition resembles the action of wit and dreams in the abrupt way in which complicated unconscious processes appear, clearly and succinctly, in a spontaneous, uncontrived manner. Like wit, intuition brings an explicit novelty to experience. It is sudden and compelling, and has the flavor of instantaneous truth. Even if succeeding events make the reality of the experience and truth of the propositions about it seem unlikely, for the moment, intuition provides an arresting conviction of reality which is similar to the pleasure derived from wit.

Intuition has long been described and highly praised as a vivid and comprehensive evaluation of experience. Compelling forms of intuition are often called "poetic insight." In less dramatic form, intuition participates in everyday feelings of conviction that guide our thoughts, responses, and perceptions, just as surely as cerebellar reflexes automatically insure our equilibrium. Intuition is undoubtedly the result of many processes, and what consciously appears as novel or astonishing is an inescapable consequence of emotional attachments and acceptances. Intuitive cognition leaps over an apparent gap of ignorance from the vantage point of long-established positions. Most conscious thought consists of the conjunction of one acceptance to another, with logical and linguistic transformations intervening to recast the new in experience into terms of the old. Thus, since there is no pre-existing model, the absolutely novel is rejected altogether. The abiding presence of the reality sense in experience is recognized, not by the

traditional question, "What is real?" but in the more imperative question, "What is real for me?" In this context, "real" means, "This has meaning for me," "This is constant, even though it moves or changes or I move or change," or, "This is consistent with what I have experienced and believed before."

There is another aspect of the intuitive process, one which uses reality testing. In this case, intuition refers to the immediate inferences which are drawn from minimal cues. In response to few data, the entire process of reality testing is activated, and, with sweeping suddenness, a proposition of high probability is enunciated. Such "inspirations" may be valid as well as convincing. Intuitive ideas result from both inductive and deductive processes and may be expressed in either general propositions or in particular instances of other hypotheses which have previously been experienced as real or valid. Later considerations may indicate that the inference was made too rashly. When this happens, pleasure vanishes, because the sense of conviction is gone. Even sophisticated thinkers frequently retain some ideas after evidence has proved them to be wrong, because they cannot discard the sense of reality first conveyed through intuition.

The final truth of an intuitive proposition about experience depends upon reality tests, as will be discussed later. Intuition is emphasized here as a process which vividly conveys a sense of reality to the structure of perceptual and conceptual information. It endows experience with conviction rather than truth, with the *feeling* of reality rather than with final reality value.

The hypothesis is proposed that reality sense is a function of the libidinal attachments of the personality and of the cathexes of the ideas and objects which comprise the ego image. The sense of reality is fundamentally irrational in that it requires no justification for acceptance since it is the nature of irreducible justification itself.

Libido

The libido theory was originally proposed in order to establish a biological foundation for the intensity and vicissitudes of emotions and instincts. Psychoanalysis has given both a general and a specific meaning to "libido." In the general sense, libido has been equated with psychic energy, the power which lends force to the emotional life. It thus becomes the mental representation of the sexual drive. However, aggressive impulses, as well as sexual ones, contribute to the cohesiveness of emotional and intellectual activities. Aggressive impulses, recent work has indicated (69, 70) are directed toward the pleasure derived from efficient performance, toward ambition and achievement, not only toward the direct expression of destructive hostility. In a specific sense, libido has also been used to refer to the qualitative sensitivity of various zones and areas of the body.

These two meanings are consistent with each other in the sense that libido is viewed as a general source of psychic energy which attaches to various parts and functions of the body, either directly, in the process of maturation and growth, or by transmutation or displacement.

In this paper, libido is used to refer to the primitive instinctual anlage from which erotic and aggressive impulses later differentiate, but never entirely separate, each from the other. Libido binds together the emotional life, giving force, direction, and form through its attachments. It endows certain organs and tissues of the body with a unique sensitivity which transcends their primary perceptive modality. The primary function of the eye is to see; but the pleasure in visual perception is a function of the libidinal investment of the seeing process and of the objects seen within the visual field. Libido is thus invested in the perceptive organ, in the bodily function, and in the object of perception. The aggressive form of libido is predicated by the integrated, efficient operation of the various organ systems of the body acting together in adaptation to social and biological stress. Aggression refers primarily to the "action" aspect of the organism, the "forward surge" of animal drive, which reaches fulfillment in mastery and accomplishment. Libido thus may be manifested on any level of psychological or biological function, from the specific pleasurable sensitivity of certain apertures, organs, and

prominences, to the highest symbolic, artistic, social, and intellectual achievements.

Libidinal orientation is determined not only by organ activity, but by fixations derived from non-hereditary sources in individual and cultural experience. Laforgue (39, 40) has indicated how oral, anal, and phallic orientations may determine a person's intellectual and emotional style and interests, his concepts as well as his pleasures. Anthropologists have made similar observations (5). Libido, through the emotions, contributes to the integration of the organism's search for gratification and achievement. In man this is not limited to the specific erotic functions, but extends to the emotional cathexes of ideas, concepts, plans, ideals, and performances. Thus, it is proper to speak of *libidinal fields* rather than of libidinal objects, since we are referring, more or less, to a distribution of cathexis, which, in one form or another, shifts and changes, concentrates and attenuates, depending upon experience and need.

The attachments of the libidinal field are not fixed, although some are more persistent than others. There is variation in degree or intensity of libidinal investment as well as in location. The concept of libidinal field corresponds, in some respects, to Lewin's "life-space." It is designed to account for both the extent and intent of emotional relationships, and to provide a uniform conceptual framework for libidinal variations and attachments.

The classic genesis of the sense of reality depicts the infant as progressing from an early auto-erotic interest to a gradual spread of libidinal attachment in the quest for gratification. Interest ultimately passes from his own separate organ functions to objects in the outside world. In this latter phase, there comes recognition of himself as a participating unit in the world rather than as its nidus. The classical description requires that a distinction be made between "object libido" and "ego libido." Consequently, libido has been thought of as a substance which passes, in the course of maturation, from the ego to objects. Useful though the libido theory has been in psychiatry, the likelihood of further modification is apparent. Libido is certainly not a specific pluripotential substance, however convenient it may be to regard it in such terms. In fact, "libido" may suffer the fate of "phlogiston," which enabled chemists to make certain observations, but which was supplanted by other concepts as investigation advanced.

The concept of libidinal field does not regard libido as a kind of transmutable metaphysical substance, however, but as analogous to fields of force, which may be displaceable, fluctuant, and variable in distribution and intensity, uniform or heterogeneous, single or multiple. The concept of libidinal field tries to account for all forms of emotional attachments as fields of force distributed about the central human organism. In the course of development, the libidinal field parallels the growth of other biological capacities. The child does not walk until the corticospinal tracts are functionally prepared; animals do not climb unless there is a highly evolved temporal lobe; language must await the acquisition of symbolic and conceptual propensities. As the growth and development of the organism allows for more extensive and differentiated experience, so also does the possibility of a wider, more varied distribution of the libidinal field produce a corresponding increase in the kinds and numbers of objects which enrich human experience.

In most accounts of self-object relationships, "object" has been somewhat loosely equated with so-called "physical objects." This represents an unjustified inference from the acceptance that somehow the "physical" is basically opposed to the "self," and that it represents "reality" of a kind which is quite distinct from mental contents. The notion that a "physical object" is basically different from a "mental object" is a highly sophisticated deduction from a mechanistic hypothesis, and is in no sense an obvious distinction. As will be shown, "objects," as well as "reality," are of many kinds. In the present context, as related to the explanation of the libidinal field, "object" is defined simply as "an object of experience," a designatum rather than a denotatum, in Morris' terminology (47). Thus, hallucinations, phantasies, ideas, images, and any other

form of perception may be included as "objects" which may fall within a libidinal field, in so far as they contribute to experience. An object, from this viewpoint, is what is perceived. Hence, it is a "sensory object." This does not imply that all objects must fall within a libidinal field. However, although there are unquestionably conflict-free areas in which the ego operates, the *absolute absence* of libidinal investment in an ego function or an object relationship cannot definitely be established. The sense of reality, by definition, applies to objects of experience or sensory objects which have varying degrees of interest and importance for each person. An experience in which we are totally disinterested is also one which does not impart a sense of reality. Patients with chronic apathy express doubts of the reality of the world, along with their indifference toward it. *For object of experience to be interesting and to make a difference to the percipient, and, therefore, to have a sense of reality, they must fall within a libidinal field.*

It is important that the sense of reality not be confused with the functions of reality testing. This distinction will be more fully discussed subsequently. Reality testing involves a complexity of perceptual and intellectual functions which conceivably can operate with scant reference to the libidinal field. The sense of reality refers to objects which are directly experienced; the vividness of the experience somehow determines the degree of conviction which it bears, however evanescent this may be. A vivid dream may force the dreamer to believe in its "reality." But when the dreamer awakens and reality testing once again functions in its full scope, the sense of reality changes and the dreaming reality is relegated to another category of mental life. Reality testing is suspended during sleep; concepts and abstract properties are reduced to sensorimotor experience alone. The libidinal field is concentrated, however, as a result of withdrawal of external cathexes. Memories, phantasies, and fragments of experience populate the dream world with the full vigor of acceptances which have sprung from the primary process. Although reality testing evaluates the dreaming experience, without the presence of libidinal fields there is no need to determine which is sleep and which is waking, for it no longer matters.

Reality sense is most vividly appreciated in self-perception. Philosophers with views as divergent as those of Descartes and Bergson have begun their reflections with the basic acceptance of the reliability and reality of self-cognition, which then becomes the model of all reality.

The self image consists of the body image and the ego image. Our conception of ourselves as a continuous personality, a feeling, thinking, acting personality, with obligations, ideals, shortcomings, and purpose, is the ego image. The body image consists of libidinized and conceptualized sensory schemata. Many different kinds of information are integrated in the total social and biological self image. Systems of ideas (acceptances), systems of images (perceptions, primary organ functions and confunctions), systems of actions (phantasies and motor patterns), and systems of libidinal fields (object cathexes and erogenous zones) participate to create the model of what we are. This model constantly changes, but it always maintains those invariant characteristics that find us each day more like ourselves than like anyone else.

The invariant experiences of self-perception include a feeling of permanence and an awareness of an ephemeral boundary which separates us from other people. There is an "I" that perceives, and an inner tension that persists and fluctuates in response to other experiences. Part of the intangible "boundary of the self" is marked off by what we can control. When we can control, there self is; when we are controlled, there the world is. Control and countercontrol are transfigurations of the self and the world.

Action and volition, like body and mind, are two aspects of control. What cannot be controlled, even if perceptually recognized as part of ourselves, is still experienced as something alien which opposes the effective operation of the self. Certain patients, for example, regard their chronic illnesses as separate entities. Male patients with duodenal ulcer often speak of their lesion and its symptoms as "she," and one woman re-

ferred to her ulcer by a man's name! Control is the capacity to alter objects and to determine experience. Control does not imply absolute authority freely to manipulate human experience, but refers to the *conviction* that the sources of experience are within us and are directly related to ourselves. This includes emotional investments, motivation, phantasies, perceptions, volition, and motor performance. What can be controlled falls within the libidinal field of the ego and thus carries a strong sense of reality. Where libido extends, there is an extension of the self; and where emotional attachments pass beyond the feeling of ego control, a sense of alienation and countercontrol appears. The familiar clinical symptom of loss of control is closely associated, in severe personality disruptions, with the sense of being controlled. Analogously, any extension of the self used to control the environment is included in the libidinal field of the ego. A hammer which is used to drive a nail becomes a part of the self in so far as it participates in molding experience. The internal extensions of the self include such factors as group memberships, national identity, and loyalty to causes. These are all endowed with libido, and, in this sense, are identical with the self. The libidinal fields of the ego include both external and internal relationships which are differentiated by the kinds of experience they provide. Only as these extensions of the self pass beyond the body image do they acquire independent status. As the wish is the prototype for the accomplished fact, the self reflects the image of reality. Systems of thought which actually interpret experience exclusively on the basis of controlled, libidinized human experience are called *animism*.

Narcissism is the libidinal field of the self. It represents the cathexis of both the ego and body images, and provides for the integration of all perceptions within a libidinal field. So-called primary, medial narcissism refers to experience within the body image; when the libidinal field passes beyond the body image to encompass other types of experience, this is secondary narcissism (secondary libidinal fields of the self image). Schilder (63) recognized that the image of the self is determined by the physiological aspects, the libidinous structure, and the sociology of experience. Federn (13) and Weiss (69, 70) proposed that the primary defect in mental illness is in the libidinal investment of the ego. In this way, the libidinal fields of the self, concentrated in the body and ego images, when distributed to the world of experience (secondary libidinal fields), provide for the sense of responsibility, obligation, and the directives of human conduct which are called the superego. Narcissism, or the cathexis of the self image, may thus extend from the most primitive blind yearnings to highly differentiated and enlightened self-interest and altruistic enterprises.

It may be argued that the sense of reality does not only apply to those experiences with a high degree of libidinal concentration, for are there not also so-called "neutral" objects and processes which enter into everyday perceptual experience? However, because the ego functions have such a strong sense of reality themselves, we believe in what is seen, heard, felt, or thought, have faith in the accuracy of perception, and have unquestioned conviction in substance, time, and space, and in the reality of ordinary, common sense objects. The libidinal cathexis of ego functions, moreover, gives us gratification in the continuous, efficient operation of our perceptual and conceptual machinery. There is belief in the acceptance that one experience is related to another. The apparently neutral objects and judgments of common sense are actually based on the sense of reality which is attached to the ego functions. This is expressed by the confidence which we have in reality tests themselves.

REALITY TESTING

The sense of reality and the tests of reality all participate in the complete reality experience. There are some emotional experiences which are so vivid that alone they carry the conviction of reality, and yet are difficult to confirm with tests. Such private experiences as pain, elation, falling in love, and certain religious or esthetic moments are of this kind. Only statements about these

experiences can be tested, not the experiences themselves. Reality testing deals with the corroboration of experience from one person to another, or from one moment to the next within the same person. The sense of conviction of reality sense is private, personal, and undisputed. It arises from the emotional structure of experience, and differs from the public, impersonal, and intellectual context which is reality testing.

Reality testing does not decide which experience is real and which is unreal, for any experience is "real" from a special point of view. A hallucination is as real as a chestnut; an atom is as real as a poem; a bird is as real as a memory. The central task of reality testing is to determine the various contexts in which different experiences may be called "real." Its fundamental aim is to make distinctions, to appraise, order, and classify experience.

Reality testing operates even in the act of intuition by means of an immediate inference from minimal cues, but there is need for confirmation and corroboration of these transient insights. For this reason, the first requirement of reality testing is the *repetition* of experience. This includes both recollected experience (memory) and controlled experience (experiment).

Perception and sensorimotor experience are important components of reality testing. However, reality tests must be able to appraise a variety of experiences and cannot be restricted to perceptions alone. Our world is populated not only by sticks and stones, but also by ideas, motives, plans, hopes, dreams, postures, and phantoms. These "realities" direct human behavior more often than do mere "material objects." An unreflective life is hardly worth living, according to Socrates; and a life with only reflection is not a life at all. Many of our motives and means and actions emerge from barely intimated concepts or hypotheses; mundane perceptions constitute only a fragment of human experience. Indeed, at any moment, man lives on many levels of experience. One of the tasks of reality testing is to distinguish one level from another.

Beyond perception there is another function of reality testing called *conception*. It is the process by which the invariant properties of different experiences may be recognized. The mutual property is the ordinal basis by which different "realities" may be indexed. Indeed, "reality" itself may be regarded as the collection of the different ordering principles of experience.

Closely related to the psychological process of conception is the problem of *definition and meaning*. Without definition, concepts and properties are private matters; communication, prediction, and precision of thought are impossible. The meaning of an object is derived, through definition, from the broader structure of experience to which it belongs. However, few definitions are complete. Partial definitions and imprecise meanings predominate in thought. Intellectual systems invariably suffer from chronic defects of scope and precision, and are often afflicted with ambiguities, paradoxes, hypostases, and conjectures. Nevertheless, it is an essential function of reality testing to determine to what extent our definitions are faulty, our meanings vague, and our knowledge thereby incomplete.

Although definition is usually considered a logical or linguistic problem, its components have psychological roots implanted in the soil of perception and conception, as well as in the other elements of reality testing. It is possible to distinguish four such components of the process of definition. These may be called *designation*, *identification*, *differentiation*, and *implementation*. In order to define it, an object or experience is usually given a distinctive name, or designation, such as "chair," "justice," "dog," "sin," "entropy," "glacier," "schizophrenia," and so forth. Identification describes the features which an object shares with other classes of objects, for example, "A dog is a mammal," "A chair is an article of furniture," "Entropy is a mathematical factor which. . . ." However, in order to distinguish an object from other objects with common properties, differentiation is required, as, "A chair is an article of furniture with four legs and a back." Finally, an adequate definition must indicate the instrumental function which an object serves in the universe. This is implementation, and is illustrated by, "A chair

is an article of furniture with four legs and a back which is used as a seat." Implementation expresses the relationship of an object to the person who conceives it, the effect that one concept has upon another, and how it may serve as a guide for action. It provides the means whereby a concept may actually be applied to further reality testing, by entering into experiments and predictions. The way in which an idea is used is frequently the chief method by which its meaning is understood.

Abstract meaning ignores the actual application of a concept and defines it merely with respect to designation, identification, and partial differentiation. Examples of abstract meaning abound in theological and philosophical literature, in terms such as "The Absolute" and "Pure Being." The purely operational interpretation of meaning, however, is almost as unsatisfactory. A chair is not merely something to sit on, any more than a heart is just something which pumps blood. There are many implemental meanings of a concept, but even if there were none, the other three concepts provide the basis for at least partial definition, and hence may carry significance. In this sense, the variable range of meaning of a concept by which it is related to other concepts is called the *conceptual field*. Meaning depends upon conceptual fields in much the same way that emotional attachments depend upon fluctuations of the libidinal fields. Meaning is not absolute, except with respect to an ordering perspective, or *index*. For example, the definition for unicorn is, "A mythical horse with one horn." Even though no such animal is likely to be encountered, the definition has meaning. The concept of "unicorn" is clear from the meanings of "mythical," "horse," "one," and "horn." These concepts may be used as guides to further reality testing and to index the invariant features of other experiences. "Unicorn" is found to have meaning in the conceptual field which bears the index, "Fabulous monsters." In its area, "unicorn" has as much significance as a horse without horns has in its own conceptual field.

Conceptual fields may differ widely, yet as long as they correspond with respect to a single index, communication and mutual action are both possible. Meaning may remain the same when asserted by different propositions; even statements in several languages may coincide in meaning. Equivalent meaning occurs and occurs only when a common ordering index is employed.

The common ordering index of experience must be shared by others. This is the important corroborative element of reality testing. What only one man can experience, regardless of how often, or with how much conviction, must be doubted, as the investigator is doubted who produces data that only he can confirm. This does not mean that the truth of a proposition depends merely upon the number of people who believe it. If this were so, then the most widespread superstition or preposterous prejudice would be universally true. Rather, corroboration means that among people with similar conceptual fields the reality of an experience has the same general structure. For example, various groups may interpret an imminent thunderstorm as an evil omen, a happy event, or a scientific phenomenon. Each group relates the experience to a different conceptual field. The thunderstorm is given meaning, or defined, by the concepts to which it is related. Aside from a limited range of meaning which all three groups share, such as, "Big noise and much water from the sky," the conceptual fields of the thunderstorm experience differ. The first group relates it to a magical system of portents, the second, to its predicted effect on crops in time of drought, and the third, to certain meteorological principles. The reality test of the thunderstorm as a conceptualized experience differs for the three groups, but the reality test which establishes it as a common experience depends upon tests which are used by all groups. So closely allied are the notions of "reality" and "truth" that it is difficult to determine where the mere experience of the thunderstorm ends and where statements about it begin. The line between fact and faith is often dim. The thunderstorm is fact; the beliefs about it are merely hypotheses which extend its range of meaning, by means of ordering and appraising the experience as it

relates to other beliefs and concepts. Different experiences may share several properties. The important invariants, however, are those which make a significant difference by being able to provide a basis for further action. The concept of "weight," for instance, provides a significant ordering index. It is a common "reality" shared alike by feathers, lead, and people. Not only can it be designated, identified, and differentiated from other relational properties, but there are conceivable actions which can be performed to establish the place of otherwise dissimilar objects within its scope.

Reality testing includes conceptual fields which extend into the future. This is called *prediction*. It also includes conceptual fields which extend into the past, called *memory*. Past, present, and future have an immediate significance for each other. In psychoanalysis, for example, a total mental event includes all aspects of duration (current situations, past memories, and future aims), and, therefore, may be treated as though simultaneously occurring within a single conceptual field.

Ordinarily, the concept of phantasy and play is contrasted with that of reality. This is because reality is conceived of as an experience beyond our control, while phantasy and play are regarded as freely manipulated and manufactured experience. It is this feature of phantasy and play, however, that is important in reality testing. The capacity for controlled and repetitious experience is characteristic of experimentation. In the same way, imagined events and actions may provide a plausible relationship between one kind of experience and another. The child plays with a spool of thread while his mother is away, as a consolation for her absence (20). If the equivalence of mother and spool can be accepted—and this is the playful, phantastic element—then the mother's return is guaranteed. Play, of course, has a wish fulfilling function which reality experience does not have. But, where this is not an end in itself, play, as a contrived and elaborate pretending, actually provides an experimental model for coping with future situations and for understanding mystifying events of the past.

Symbolization is that mental function which permits the development of language and communication on a higher level than that permitted by perception and conception alone. It is different from the so-called unconscious symbolism of the primary process. By means of distinctive symbolic forms, various classes of objects may be defined and different kinds of experiences may be related to others in unique terms. Symbolization provides a common index, a common symbolic form, and a common language which relate otherwise differing conceptions and perceptions. Propositions, assertions, and judgments, which relate invariants of different experiences to an abstract symbolic system, participate in that aspect of reality testing called *logical thinking*. Reality testing requires the use of statements which may be judged true or false by some reference to established criteria. Truth about propositions is determined either by consistency with already accepted statements, or by correspondence with other perceptual experiences. In the domain of logical thought, reality testing becomes verification; the problem of reality and its conditions becomes the problem of truth and its criteria.

In summary, reality testing consists of the various intellectual processes which are capable of describing and judging the conditions and contexts of experience. With the development of perceptual schemata, conceptual fields, symbolic forms, and logical systems, a wide variety of experience on different levels may be appraised. Reality testing, when dealing with either single or repetitious, shared or unshared experience, operates on several levels of abstraction, ranging from perception of the invariant properties of everyday objects to the validation of logical consistency between propositions.

Assertions made about reality are propositions. These are judged as either true or false, depending upon the conceptual framework to which they are referred. Validity refers to logical consistency and is the reality test for propositions beyond direct experience or judgment. Finally, there must also be belief in the actual processes of reality testing, for reality testing is a product of

four general mental functions: perception, with belief in change, motion, or contrast; conception, which determines the invariant common properties between distinct events; symbolization, which expresses certain experiences in terms and functions which are not a part of the original event; and logical thought, which develops systems out of symbolic forms. The final aim of reality testing is to define objects of experience on whatever level they occur. It is in the fact of belief—the confidence we have in our own acceptances and the willingness to take action on our ideas—that reality sense and reality testing ultimately are fused.

Although reality sense and reality testing have a common source and aim, there are certain contrasting characteristics which may be summarized. Reality sense deals with whole situations, rather than with parts of experiences. It is synthetic, rather than analytic. Reality tests are intellectual, rational, and conceptual, while reality sense is emotional, intuitive, and perceptual. For reality sense, objects are complete events in themselves, absolute and unequivocal. For reality testing, objects are parts of a wider context, whose relations must be determined. Its results are only approximate and conditional. Reality testing finds its material in the conditions of experience, while reality sense functions with respect to the experience itself. Reality testing deals with shared experience through the common meaning of various propositions about it. Reality sense is essentially unshared, private, and complete in itself; it has no need for confirmation, since its criterion is the intensity, rather than the invariants of experience.

DISSOCIATIVE DISORDERS OF REALITY SENSE AND REALITY TESTING

Many psychiatric patients undergo baffling and frightening experiences, due to a dissolution of harmony between the beliefs derived from the sense of reality and the knowledge obtained from reality testing. There is a lack of coordinated interaction between reality sense, with its emphasis upon conviction and the intensity of emotion, and reality testing, which interprets experience on the basis of a conceptual system. In melancholia, for example, there may be a vast discrepancy between the patient's self-derogation and his actual offenses. Yet, although this is often recognized, the patient is unable to brighten the grim and hopeless shades with which his mood has painted the pattern of his life. Memories are not initially falsified, but the depression so distorts recollection that even the most trivial actions are accepted as substantiation of the self-incriminating attitude. Unconscious libidinal cathexes determine the reality sense of depression, and reality tests are unable to temper the conviction of absolute degradation. The self-destructive urges of depressed patients seem inconsistent with the tendency of healthy organisms to adapt to stress in an optimal way. But suicide represents an effort to achieve regressive reintegration of reality sense and reality testing, and to find harmonious adaptation between knowledge and belief. The depressed patient attempts to destroy that part of himself which is the source of conflict, and thus preserve an intact ego image, even though in a painful form. Masochists may arrange to suffer in order to master guilt feelings, or they may neutralize sadistic impulses by undergoing constant defeat. In effect, masochistic characters must suffer in order to feel real. Because guilt represents the inability of the ego to fulfill the prohibitions of the superego, a depressed or masochistic patient often believes that only through self-abasement, even self-destruction, may an unacceptable self image, which has been determined by the sense of reality, be integrated with the reality testing functions of the ego and the superego. As pain indicates the need for adaptive measures on the somatic level, so guilt and depression are signs of adaptive needs on an unconscious psychological level. When guilt results only from a conscious misdeed there is little discord between reality sense and reality testing; the punishment fits the crime. But when the unconscious libidinal field is at odds with the conceptual field of the ego and superego, the dissociation leads to feelings of guilt and depression and, in many instances, to depersonalization (51)

in order to establish a harmonious ego image and to fuse reality value. The patient's need to feel whole, to experience reality, requires that he feel guilt.

In patients with obsessional neurosis, reality testing often seems to be enhanced at the expense of reality sense. This is particularly evident in the doubting compulsion. These patients believe nothing. They can make no assertion without immediately negating it. Emotions are rarely experienced. For the most part, such patients are restrained, isolated, and affectless. Indecision accompanies doubt, for the patients unconsciously recognize that no action, no alternative, will bring fulfillment. Even everyday perceptions are doubted. Parts of the body and familiar objects of the environment are viewed with such detachment that if another person should claim these things, the patients would not be surprised, so alienated are they from themselves. Intellectual values replace emotional values; for the patients who must doubt and refuse to commit themselves to one action or another, reality value is ostensibly determined by reality tests alone. The only guide to action is through intellectual considerations which may be logically consistent, but which carry no conviction. Instead of root beliefs, these patients face the world armed with, "If . . . but . . . except . . . however." The emotional components of experience are so often obscured with the dust of doubt that any conflict may be readily acknowledged, because the pain, anxiety, or guilt cannot be consciously perceived. The libidinal attachments to earlier experiences emerge only in symptoms and defenses. Sometimes gratification may be found in the persistent refusal to fulfill instinctual urges. The aggressive puritanism of some patients amounts to a lifelong vice. Sometimes certain intense experiences, or even deviations from the habitual routine, can abruptly activate the libidinal field. In such instances, the eruptive guilt or anxiety may be neutralized by doubt and depersonalization which spread over the face of the self image, as in Case 1.

Case 1. A 32-year-old man with obsessional neurosis decided to break his rigid work schedule to go to a movie in the afternoon. He chided himself both for the whim and for the intolerant attitude which made him guilty for his peccadillo. While at the movie, the patient became engrossed in one of the leading characters, a ship's captain, who had lost his family through death and was as filled with hate and revenge as he had once been with love. The captain was alienated from every mundane ambition and material gain; he was dedicated only to the merciless destruction of the people he despised.

The patient recalled that the night before he had had a series of dreams involving bloody scenes in a slaughter house. As he walked immaculately through the carnage, some blood spattered on his clean suit. He was, however, only mildly annoyed, as a man might be if gravy had spilled on his necktie. He was totally indifferent to the bellowing, blood, hacking, and death that surrounded him.

The captain's antagonist in the movie was a boisterous, insensitive, conventionally self-righteous sailor, who wanted to destroy the captain, assume his place, and become rich. It was clear that the sailor would ultimately triumph. As the captain's power waned and his authoritarian control disintegrated, the patient became increasingly anxious, even somewhat panicky. He felt helpless, swept along by the tide of events, as the captain's fate was foredoomed. Suddenly, there was an intense resurgence of guilt about breaking his daily routine, and then, all at once, the theater seemed unreal and strange. He felt detached and alienated. The other patrons, as he looked about, seemed to be wretched vagrants, filling their empty lives with vicarious adventure. The exciting movie began to bore him; the anxiety and guilt disappeared simultaneously. He left the theater without seeing the end of the picture. The streets seemed as unfamiliar as his bodily sensations were alien. He returned to his usual routine, but even familiar actions felt strange. Within an hour, however, the unreality feelings faded, and he was immersed in his rigid life again.

During this brief episode, the patient's reality testing was intact. He was fully oriented and aware of his altered feelings. His sense of reality, however, had been affected, for the movie events seemed to be more real than his own life. Clearly, there was a parallel between the captain and himself. The dream reflected not only sadistic impulses, which he controlled by the immaculate means of reaction formation, but even presaged the sense of detachment which his unreality feelings expressed the following day. By the willful act of attending the movie in opposition to his rigidly controlled life, the patient found a token discharge for his own destructive feelings. His guilt about

deviation from routine, far out of proportion to the actual offense, corresponded more appropriately to the slaughter which was depicted in the dream, to which he was manifestly indifferent. The movie characters drew his repressed conflict into imminent consciousness. He identified himself with the captain, who was both destructive and benevolent, and as much a victim of ambivalence as was the patient. As a result, he became panicky when the captain seemed destined to be destroyed. His guilt and anxiety could no longer be held in check; the libidinal field, aroused by the dream and kept active by the movie, then controlled his reality sense. The audience became a horde of vagrants, a projection of his own ego image. The movie was a kind of second chapter of the dream, in which sadistic phantasies and his guilt were revived. He felt alienated from the waking world because his sense of reality adhered to the dream world. Through the dissociation of intellectual processes from libidinal orientation, for a brief period the patient lived a waking dream.

Hysterical conversion symptoms may also be associated with unreality feelings. If certain phantasied behavior is repressed, sometimes the significant parts of the body engaged in the forbidden act will undergo functional repression, as in Case 2.

Case 2. A 27-year-old musician complained of feelings of estrangement during concerts. This chiefly involved his right hand, which felt strange and uncontrollable, although he was able to perform satisfactorily from the audience's standpoint. At one time he was unable to play because of total paralysis of the entire right arm. In addition, prior to each concert an uncanny feeling of guilt descended upon him. This could be relieved if he could feel that he had played poorly, whether or not this was true in fact.

His musical career had been directed by an ambitious mother who controlled his personal life, even after he became an adult. Many years before, all pleasure and spontaneity had disappeared from his artistic life and he felt "burnt out," after a promising beginning as an adolescent. His overt attitude toward his mother was one of mild, stale resentment, if not total indifference. He was superficially compliant to most people. However, psychoanalytic study disclosed that each time he was called upon to perform, dormant anger was awakened. He became panicky, as his homicidal impulses clamored for recognition. The musical performance had become associated with his long repressed hostility toward his mother. These feelings were frequently followed by a sense of estrangement and a feeling of numbness and clumsiness in his right hand. The anxiety, at this moment, yielded to guilt.

The patient's conscious resentment was merely a relic of his past anger toward his mother, which had been controlled through the years by denial, inhibition, and reaction formation. The symptom of anxiety and intense guilt, followed by unreality feelings and alienation of the right hand, could be relieved by failure. This was because the successful use of the hand was involved in the unconscious phantasy of killing his mother. It was more acceptable to fail as a violinist than to succeed as a murderer. The right hand felt unreal during a performance because it was controlled by the libidinal field and reality sense of the past. The libidinal function of the hand dominated the musical function, as well as the reality testing of the present.

Distorted reality sense characteristically controls reality testing in other clinical disorders, such as transvestism, fetishism, and some forms of masochistic perversion.

Some ego defenses result in a dissociation of reality sense from reality testing. For example, in denial, whether by word, act, or phantasy, the patients assert that a certain attitude is not the case. They either seek other explanations for their feelings, or repudiate their ideas altogether. This means that the patients recognize certain emotional cathexes which involve themselves, but find it necessary to disclaim the corresponding conceptual field, so they substitute another. In rationalization, the patients seek conceptual justification for a course of action toward which they are already emotionally predisposed.

States of elation, as well as feelings of depression, guilt, and anxiety, may result in dissociated reality testing. This may come about spontaneously, or through the use of drugs. In the following case, the combination of large amounts of amphetamine and exalted phantasies glamorized painful realities to the point of ecstasy. The libidinal field was altered both by the drug and euphoric denial, thus transforming current

experience so completely that reality testing led to erroneous appraisals, and fact was confused with fancy.

Case 3. The patient was a 50-year-old mechanic who, despite an excellent education, had advanced only to a minor shop position. A diffident attitude had paralyzed any actual striving for advancement, and he accepted his humble status without apparent distress. His chief concern in daily life was to follow unquestioningly one order after another, and to maintain a detached and emotionless aloofness toward those about him. Behind this humble facade, however, the patient spent hours spinning out elaborate phantasies of success and triumph in order to neutralize his utter social and emotional poverty. These daydreams so enraptured him that ordinary social and sexual experiences seemed inconsequential by comparison.

Several years previously, a physician had prescribed amphetamine as part of a weight-reduction program. The glow which the drug produced so altered his perception of everyday life that even his engrossing daydreams were given up. It was as though the amphetamine provided a waking daydream in which substitutes were no longer necessary. He worked, spoke, and behaved as if he were in fact the overwhelming success that in the drug-free period he could only dream of being. During the exalted phase, however, he began to phantasy constantly about being beaten by a large woman. He pictured himself as helpless, forced to lie between her buttocks. There was no pain, only an exquisite pleasure in complete submission.

Actually, the patient had had few overt heterosexual experiences. Not even under the effects of amphetamine did he seek fulfillment of the beating phantasies; he sought only to prolong them. Significantly, the patient's obese mother had had a postpartum depression and was forced to leave him for the first eighteen months of his life. He had, of course, no recollection of his early life prior to his school days. It was clear, however, that during his drug-induced delirium, reality sense was modified, and the latent yearning for his mother was revived. His perversion in phantasy represented a regressive return to the libidinal field of his infancy, which had been hidden by the perpetual neutrality of his everyday life. While using the drug, he did not need success; he was, pro tempore, the infant lying between the buttocks of the fat woman his mother had been when he was born. This was the one enduring "reality."

The importance of an intact body image for the perception of reality is illustrated by two related "neurological" syndromes, each of which demonstrates a dissociation of reality sense and reality testing. These two disorders are *phantom limb* and *anosognosia.* Frequently, following amputation of a limb, a patient may feel that the absent part is still present, and may even describe various changes in its shape and position. Such responses are usually transient, but the painful phantom limb is apt to present a most refractory problem. According to Kolb (38) the phantom limb persists after amputation because the body image remains intact. As a result, fresh perceptions are interpreted against the background of the preoperative body image and sensory schemata. Kolb found that certain emotional factors, such as close relationships with amputees in the past, may perpetuate phantom limb perceptions. Although the persistent sensations which arise from severed nerve connections are still interpreted in terms of a conceptual field which involves an intact body image, the emotional attachment to the amputated part, especially as it relates to disturbing recollections and phantasies, leads to a dissociation of reality sense from reality testing, which remains more or less undisturbed. Hence, the patients are often completely aware that the body part is missing, although they feel as though it were still present. Indeed, it is often this perplexing fact itself which causes much of the distress. The persistent painful phantom limb represents an effort to maintain an intact ego image, by the preservation of a body part in a libidinal field of suffering.

In a sense, anosognosia, or denial of illness involving a body part, is the opposite of the phantom limb phenomenon. The patient denies any impairment of body function, although he usually has suffered a left hemiplegia and hemihypesthesia. In severe cases, the patient even claims that the affected limb belongs to someone else, so complete is the alienation of the extremity. Weinstein (67, 68) has indicated that patients who deny the presence of illness also show lifelong tendencies to avoid emotional conflict through the ego defense of denial.

Some workers (10, 12, 48) have stressed the importance of the thalamoparietal peduncle in the syndrome of anosognosia and amorphosynthesis. Bender (3) and his associates have studied disturbances of perception which affect reality testing. Their investigation of sensory extinction disclosed that there are many perceptual defects which are overlooked by conventional neurological examinations. Sensation which is not perceived on the affected side, for example, may be displaced to the corresponding area on the intact side of the body (allesthesia). With more severe cerebral damage, the perception may even be projected to a source beyond the patient's body image, and the patient may then react as though his affected extremity belonged to someone else. Bender maintains that perception is not a simple process, but is a component of a wide pattern of relationships between the external background of the stimulus and the internal field of the patient. Sensory schemata and habitual conceptual and emotional responses belong to this internal field. Perceptions are not possible without conceptualization. Hence, when cerebral disease produces a disturbance of perception there is also a disorder of the conceptual field, symbolic forms, and the relationship between them. There cannot be appropriate response or familiar recognition of stimuli if the cerebral disorder has altered the background to which these stimuli are habitually referred. This indicates that cerebral defects may produce a disorder of reality testing which is then dissociated from reality sense. Severe lesions may be associated with permanent, gross defects of reality testing, as in cases of agnosia and apraxia.

Sometimes an intact reality sense overcomes reality testing in other paralytic illnesses, and delusional beliefs may occur. Some victims of poliomyelitis who are almost totally paralyzed develop deliria in which scenes of strenuous movement are prominent (9). This represents a regressive dream-like denial of the paralysis by virtue of the enhanced libidinal field pertaining to motion, accompanied by a denial of the evidence of reality testing. Since delusions are, after all, misplaced concepts, or errors in appraising experience, it is evident that the intense emotional response to the fact of paralysis overcomes reality testing, and produces "phantom motion."

Reality testing may be dissociated not only from reality sense, but different components of reality testing may be dissociated from each other. Vertigo offers an interesting example of this phenomenon. The vertiginous patient reacts to a dissonance between labyrinthine sensations and visual perceptions with a false sense of movement. Some episodes of vertigo are "objective," that is, the patients feel as if the world is turning in a fixed direction while they remain stationary. They can relieve much of their distress by eliminating the incongruous visual perceptions; they lie in a darkened room with their eyes closed. Other patients experience "subjective" vertigo. They feel that they are rotating while the world is stationary. Some of these patients are helped by focussing on fixed points in the environment and by minimizing vestibular sensations by lying motionless. The factors which determine each form of vertigo are not clear, and both may occur in the same patient. The illusion of false motion may be provoked by actual alterations in perceptual information, as in motion sickness, or by disturbed perceptions resulting from labyrinthine and ocular disorders, as in Meniere's syndrome or in heterophoria. Sometimes vertigo may be induced by a challenging emotional predicament which affects the "equilibrium" of the personality with respect to its adjustment to a certain environmental situation. In such cases, auditory and visual cues assume a heightened emotional significance. The question of which perception—vestibular or visual—is "believed" in vertigo cannot be answered, for it is the disruption of information from two reliable sources which provokes the loss of equilibrium. Vertiginous patients often develop feelings of estrangement as a psychological adaptation to their perceptual dissonance. Presumably, the organs with the most intense cathexis, and which produce highly differentiated sensations, play the most significant roles in the development of the body image. Schilder believed that prominences

and depressions in the body surface have the most libidinal concentration. Bender demonstrated, however, that certain areas, such as the face, have both perceptual and emotional dominance over other areas of the body. The sensory modalities which provide the most differentiated perceptions of the social and physical field probably contribute proportionately to the ego image. This includes vision predominantly, but hearing is also important. In other animals, the sense of smell is enhanced, and an exquisite differentiation of olfaction is possible. The native predispositions of certain organs to perceptual dominance may be modified through various experiences, such as trauma, fixation, habituation, conditioning, and training. The asthmatic patient's respirations, for example, share a more prominent libidinal field than do those of people without a breathing disorder.

The libidinal field may not be concentrated only in certain physiological functions and dominate the reality testing pertaining to that function, but, in some cases, there may be a special libidinal concentration in selected psychological areas, which leads to a focal disturbance of reality testing. This may be seen in the following case of pathological jealousy. The patient had "partial insight," in that he recognized the unreality of his beliefs, yet he was helpless to control an overwhelming mistrust of his wife.

Case 4. A 47-year-old executive, who suffered from periodic depressions, became convinced that his wife was unfaithful to him. He recognized that there was no evidence to support his suspicions, but, in his troubled state, was certain that each time he was away from home, his best friend visited his wife. Even when she was delayed by an errand, or merely in another part of the house, he was momentarily sure that she had been with the other man. He constantly sought evidence of her infidelity, and made many surprise trips home to set traps for them. Even though none of his schemes provided the slightest confirmation of his delusion, he persisted in working out still more elaborate plans to discover what he was convinced must exist.

Other than in this circumscribed sphere, the patient functioned successfully. In his history, there had been several other episodes in which he competed with another man for a woman. These affairs usually terminated in his rejection. He had been unusually close to a younger sister. When she married he became estranged from her, and, professing total indifference, had seen her only a few times in the intervening years. In early adolescence, there had been overt incestuous acts between them, which were discontinued when the sister began to date boys and have an active social life of her own. Following this, the patient was alternately critical and affectionate toward her until the final break at the time of her marriage. Toward the rest of the family he was either indifferent or spiteful.

In the course of psychotherapy, the patient became aware that his suspicions were unreal, particularly when similar ideas cropped up in relation to the doctor and his wife. This degree of "insight" served only to augment the intensity of the delusion, rather than to relieve it. Further investigation indicated how his unresolved incestuous relationship with his sister had survived the frustration of years and their respective marriages, and had been carried into his marriage. This pre-existing libidinal field lent a reality sense to his phantasies and overcame his reality testing in this particular conceptual field. The meaning of his marriage was altered to become, in effect, a resurrection of the incest. The conviction that his best friend and wife were deceiving him was a repetition of the way in which he and his sister had deceived their parents, and later, of how his sister had been unfaithful to him by turning to other men.

This case illuminates certain features of delusional thinking, which may be defined as an error of indexing of conceptual elements in experience. A similar dissociation of reality sense from reality testing may come about through impaired intellectual processes, as in deliria or organic brain syndromes. Or, as in most cases which the psychiatrist is called upon to treat, special unresolved conflicts may distort the libidinal field (6, 57, 58). When the doctor asserts that the patient's reality testing is defective, he means that the patient does not interpret common experiences in the same way that the doctor or, presumably, other healthy members of the community would. Yet, how difficult it is to understand the personal, libidinal features of reality testing which have distorted the conceptual appraisal. The doctor can only reclassify his patient's be-

liefs, and, by clarifying the reasons for the uncorroborated assignation of ideas to one category rather than another, bring about a reappraisal by the patient.

There is neither absolute discord nor constant harmony between reality sense and reality testing. Reality sense or reality tests in one area of mental life may be opposed by reality sense or reality tests in another. Everyday life provides many examples of fluctuating dissociation. Superstitious belief in magical forces which determine human destiny is countered by belief in the evidence of our own senses and in the reality of cultural myths. Willingness to accept authority without dispute is often neutralized by parallel recognition that everyone, to some extent, must answer problems for himself. The same dream, which seems real enough during sleep, may be ridiculous when the dreamer awakes. This common dissociation occurs because fully active reality testing is suspended during sleep; conceptual fields are restricted to fragments of sensorimotor experience, and abstract ideas are concretely expressed. The reality sense of bits and scraps of unresolved experience, which the reality testing of waking life neutralizes, is enhanced during dreams. Not only does reality sense influence reality testing, but reality testing also modifies the direction and degree of libidinal attachments. Even when fully awake, there are dilemmas, paradoxes, and fallacies which are maintained without apparent conflict. Which of several opposing ideas, each equally credible, should be acted upon? Indecision, reversal of conviction, and irrational loyalty to ideas and institutions flood mental life. This emotional dispersal, together with logical errors and semantic confusion, leads to defective reality testing on the highest symbolic level. Correct reasoning and logical thought decide only a fraction of the ideas which guide human action, since at this highest level man may not be aware of the failure of reality testing, while the force of his emotional predispositions sweeps him forward in a tide of undiminished conviction.

IMPLICATIONS FOR PSYCHOANALYSIS AND PSYCHOTHERAPY

The processes and criteria by which we intuit and test the various events and objects of the world involve issues which encompass the entire domain of belief and knowledge. Surely, no final answer may even be submitted. Yet psychoanalysts are daily confronted with just such problems. In order to understand his patient, the doctor must search for the archaic basis of presumptive beliefs; he must fathom the manifold models by which each experience is evaluated; he must discover the cryptic guides to action by which satisfaction is attained. Within the structure of psychoanalysis itself, there are many fundamental dissociations of reality sense and reality testing which most patients undergo, and which require the analyst himself vigilantly to exercise his own reality testing. Common instances of dissociation occur in free association, insight, interpretation, transference, and acting out.

The fundamental rule of psychoanalysis requires that the patient uncritically communicate his spontaneous feelings and thoughts. The various items of communication, verbal and nonverbal, are regarded as elements of a total psychological event. The task of the analyst is to order the many separate elements within the framework of a larger pattern, and to comprehend the structure of the personality on the basis of the serial events in which the patient participates. Because it is impossible either to communicate or comprehend "everything," psychoanalysis is, perforce, only a painstakingly constructed mosaic of symbolic equivalents of events and token discharges of emotions, mediated by a meager mutual language. A vast hierarchy of experience enters into this project. Percepts, concepts, symbols, judgments, acceptances, and logical formulas blend with blind impulses, dilemmas, ambiguities, and irrational attitudes. These are only a few of the means by which the patient depicts himself. Reality sense and reality testing, for both the analyst and the patient, must operate within these various forms of communication. Through his interpretations and confrontations, the analyst expresses his own appraisal of reality, which is often in contrast to that of the patient. But the analyst must link his observations to those of the patient, so that the uncorroborated reality testing is gradually modified and re-evaluated. Inter-

pretations must not only accurately conceptualize the patient's experience and be consistent with other interpretations, but, in order to carry conviction, they must also appeal to the patient's reality sense. Without belief, understanding is undermined, and interpretations become merely plausible, rather than true.

It is also essential for the doctor to have a reality sense for his own observations as well as to subject them to his reality testing. This is conspicuously evident in the treatment of patients who use the defense of isolation. It is difficult to recognize what these patients really believe, to comprehend the range of their libidinal fields and variations of reality sense, to the extent that the analyst himself sometimes feels no sense of conviction, and, as a result, psychoanalysis becomes largely an intellectual exercise. Effective interpretation must influence conceptual and libidinal fields together, and modify the patient's reality sense and reality testing. There may also be a dissociation between these factors in instances other than that concerned with isolation.

Without reality sense, even well-founded interpretations may be shrugged off by some patients, while "inexact" interpretations which would not stand up to reality testing may have lasting therapeutic benefit. However, although reality sense is indispensable, the therapeutic experience must be grounded in valid reality testing. The emotional experience of therapy also needs intellectual and conceptual dimensions. This fusion of emotional and conceptual factors constitutes *insight*. Insight is neither intellectual dogma nor poetic adumbration, but is a congruence of reality sense and reality testing. The so-called "timing of interpretation"—the opportune moment for the analyst to make an interpretation and thus to provide insight—depends not only on the analyst's conceptual grasp of the problem, but upon his intuition of the dominant libidinal field at the moment of his understanding. Interpretation is like insight in that it involves the complementary operation of reality sense and reality testing. It is a fusion of intellect and intuition.

It is in the *transference neurosis* that reality sense most frequently is disseverated from reality testing. Transference itself begins with a specific attitude of the patient toward the doctor, to whom he attributes certain aims, motives, and libidinal interests. The prototype of the transference neurosis is found in earlier attitudes toward other important figures. When the transference neurosis is fully developed, the analyst is regarded as both an old and a new object, so that the patient may react to current situations with the reality sense appropriate to past experiences. So strong may be the libidinal attachments to early events that reality testing is often set aside, and, as a result, distortions are frequent. The patient misinterprets the data of reality testing, and makes false predictions and perceptions about the analyst. Psychoanalysis must provide the conceptual framework for the libidinal misplacements, so that the patient's reality testing may reorder his experience.

No therapeutic problem requires more ingenuity in order to re-integrate reality sense with reality testing than does the phenomenon of *acting out*. "Those who cannot remember the past," Santayana has said, "Are condemned to repeat it" (61). In the transference neurosis, the patient predicts certain behavior by the analyst. Similarly, if the analyst understands the patient's problems, he may often predict the manner in which the patient will act out. Here, too, earlier emotional and conceptual attachments are revived; libidinal fields rooted in the past find anachronistic fulfillment in the present. The need for recurrent gratification or resolution of ancient conflicts is so urgent that reality testing is momentarily obliterated. Conceptual restraint is ineffective; communication is reduced. Action and gratification supersede language and understanding. Commonly, the patient who habitually acts out is also a highly verbal person, rather than one who is inhibited and constrained. His verbalization is a substitute action; words are used to act out in the therapeutic situation, rather than to enable him to understand the source of his conflict. Acting out is seldom modified by reality testing. The libidinal fields responsible for acting out are not changed by conceptual fields without

providing alternative gratification or partial neutralization by the reality sense of other libidinal fields. Only then can acting out be conceptually integrated with other emotional attachments.

The therapeutic process has a twofold task. The various libidinal fields which yield the sense of reality must be integrated with the conceptual perspective on which the patient bases his reality testing. Reality tests and reality sense are brought together in the course of therapy to expand the fields in which they already operate harmoniously and without conflict. This union constitutes mental health.

THE NATURE OF REALITY

The problems of reality have always fascinated and perplexed philosophers. Seldom has agreement been reached, and each philosopher must begin anew to clarify his beliefs. This does not mean, however, that he deals with baseless problems. Instead, tenacious philosophical disputes demonstrate the endless difficulties encountered in the attempt to explain experience in purely intellectual terms. One of the persistent problems of philosophy has been to discover what is real in the world (metaphysics) and how we know it is real (epistemology). Such questions are meaningless, however, unless they can be more completely defined, and are unanswerable if specific methods of investigation are not available. "Reality," without further qualification, is an empty notion, a mere linguistic abstraction, but when psychologically considered, these ancient philosophical problems of reality may be transformed into operationally significant questions. Instead of asking, "What is real?" we may restate the question as, "What kinds of experience have reality value?" or, "Under what circumstances do we believe propositions about experience and judge them to be true?" In their new form, the questions have several implications. One is that what is judged true or real in some situations may be false or unreal in others. Another is that the meaning of "real" is no longer fixed and absolute; in order to have any significance, a definition of "unreal" is also required, much as "true" is significant only if "false" also is significant.

In the past, the quest for reality, or, in our terms, for the conditions of reality value, has often been a matter of intellectual schematization. Philosophers have been concerned primarily with concepts, abstractions, and consistent relationships. Such rationalistic views of life appeal more to reality testing than to reality sense. Although some philosophers, such as Bergson and Whitehead, have stressed the importance of intuition as a means of discovery, in many instances philosophy has left the sense of reality and the feeling of conviction to the poets. With few exceptions, philosophers have followed one of two paths. Those who stress reality testing have largely ignored the direct personal experience of reality, while others, who recognize the importance of reality sense and intuition, have minimized the conceptual structure of reality. This has led to the ancient conflict between immediacy and intellectualism, an either/or controversy in which various schools of philosophy refute, rather than convince, each other, in an attempt to resolve the problems by referring to the deficiencies in their opponents' views rather than to the validity of their own evidence. The history of philosophy has been characterized by the dichotomy between realism and idealism and the efforts at reconciliation. The proponents of the primacy of knowledge and thought question the concept of "substance," while the so-called realists find it difficult to account for the vagaries of man's cognition and emotions, his errors and predispositions.

Philosophical schools, like people, must begin with certain acceptances, axioms, or "first principles." Even the most closely reasoned and critical studies depend upon a body of assumptions or predispositions which are merely asserted, not proved. These basic acceptances, held with such solid, uncompromising belief, transmit some of their innate conviction to the inferences and conclusions which are drawn from them. Conceptual systems and logical proofs are convincing only in so far as they approximate the vivid faith in "first principles." Propositions may be highly probable, reasonable, and winning, but, despite this, have an "almost, but never quite" quality which

contrasts with the immediate, absolute acceptances and rejections found in the give and take of everyday life. What intellectual hypothesis can account for the unshakable conviction we have in our own reality or for the spontaneous faith in the abiding continuity of the self? Because we may not be able to explain certain experiences, this means that our conceptual systems are faulty, not that those experiences must be ignored. Actions may be determined by motives of which we are unaware. Similarly, from the psychological viewpoint, first principles are believed without proof. In spite of vigorous opposition, the appeal of psychoanalysis and its steady advance suggest that psychoanalytic insights reach certain unacknowledged first principles which other viewpoints do not comprehend. As a result, the range of meaning and primitive sense of conviction of basic psychoanalytic concepts can account for events which otherwise would seem disjointed and irrational.

Traditional philosophy usually deals with reality as though it were a well-established end product, there to be discovered by those sophisticated or subtle enough to comprehend it. In contrast, the psychologist must start where the philosopher ends, with the varieties of everyday experience, and only much later does he attempt an over-all explanation. Philosophers tend to construct closed systems, while psychologists organize observations in accordance with limited, open hypotheses. Psychological analysis proceeds from various sensory objects, thoughts, urges, and schemes, and seeks a common ordering principle for all of them. For this is reality—the sum of all perceptions which are believed and all propositions which are held to be true. The objects and events which clutter up the world are like strands of a vast fabric; chairs and shoes and steamships; atoms and enzymes; symphonies and sculpture; hopes, motives, instincts, and protoplasm; families and nations; drugs, gods, prejudices, games, dogmas, and dreams. Experience is wide, and the categories of reality must be equally comprehensive. Significant statements can be made only about real objects, not about abstract "reality," and the account of real objects (and unreal objects) must explain how an experience which is real to one person is unreal to another, and how what is real now may be real in a different way later on. The key to reality value is found in this phrase, "real in a different way." It depends upon two related factors, the *relativity of the real* and the *dualistic character of thought.*

The relativity of the real refers to the manner in which the reality value of certain objects or events may vary, while that of others remains fixed. The statement that an object is real, then, means that it is real with respect to certain fixed criteria, and unreal with respect to others. For example, a table is real in everyday life, but it is unreal when viewed in the conceptual field of atoms and electrons. The relativity of the real means that reality value is not an absolute property which some objects possess and others do not, but that reality value is a judgment, based on a special frame of reference.

The dualistic character of thought is a companion concept. Although we may recognize rationally that it is the organism which is real, our thought somehow always differentiates *mind* from *body.* Indeed, so completely do our habits of thought distinguish between the mind and body that the characteristics of one amount to an antithesis of the imputed characteristics of the other. When a specific meaning is given to any event, there is also an understanding of what meaning it does not have. Such paired, polar concepts are commonly encountered. The idea of "change" is opposed to "permanence," but each idea derives its meaning from the other. A "variable," for example, is possible only if there is also a "constant." Change and variability are as much a part of reality as constancy and invariance. The concepts of structure/function, content/form, purpose/accident, and movement/rest are further examples of how bipolar terms are used to describe the structure of experience. There are also bipolar psychological terms, each of which is used to clarify the meaning of the other. Love shares a common root with hate. Sadism is another aspect of masochism. Activity and passivity are defined with respect to similar conditions. Inasmuch as each bipolar term is a counterpart of the other, one form must have a

constant meaning for us to understand the varieties of the other.

These two principles, the relativity of the real and the dualistic character of thought, are illustrated in the various contrasting meanings of reality and unreality which have been proposed from time to time. One of the most popular distinctions, for example, regards real and unreal as synonyms for objective and subjective. This view considers that subjective experience is too personal, private, and evanescent to be real, while what is objective is thought to be impersonal, public, and permanent. Objective information, according to this hypothesis, is real because it is public property, representing tangible matters which are universally encountered, rather than experienced by a single person. When a doctor asks, "Does this patient have real pain?" he is drawing the distinction between subjective and objective as a criterion for reality. His inquiry actually asks if the patient's subjective experience of pain can be objectively verified, presumably by collateral tests or by the reports of other patients with similar lesions. This view of reality is based on the supposition that what can be "objectively" determined has greater reality value than private experiences, however intense. It also implies that if "objectivity" cannot be established, then the subjective aspect does not exist, and hence, the patient's pain is somehow "unreal." Subjective states, such as pain (64, 65) however, may represent the epitome of reality for the afflicted person, even though his physician may dismiss his complaint as imaginary. The dichotomy between subjective and objective does not adequately account for reality and unreality. It is based on a confusion between the thinker and the object of thought, which denies reality to the thinker and attributes it wholly to the object. The doctor denies the reality of his patient's pain because he defines reality *only* in terms of the object, namely, the lesion and information about it which collateral sources provide.

As with other bipolar terms, subjective and objective are not basically opposed. They merely represent different ways of expressing the conditions of experience. Both are real, for any experience is objective, or, for that matter, subjective, from a special viewpoint. An intoxicated man may be said to be objectively drunk in so far as the alcohol content of his blood is concerned, because blood may be subjected to a variety of corroborative tests. The mental state of drunkenness, however, is called "subjective" because it is experienced only by the victim. Yet both the drunkenness and the elevated alcohol content of the blood are components of the reality called intoxication. It is clear that there is no absolute distinction between subjective and objective experiences. There are only differences in judgment about the conditions which give rise to each experience, whether these conditions are determined by the laboratory, the interview, or by self-observation.

Reality testing is concerned with the most abiding elements of experience, those which may be readily corroborated. In contrast, reality known through reality sense is not necessarily so restricted. Reality is commonly regarded as synonymous with the constant, continuous, predictable elements of experience, while to unreality is consigned the transient, introspective fragments of experience, however vivid, which cannot be corroborated or repeated. However, most objects and events may neither be completely corroborated nor be completely unique; they are "relatively real." The key phrase, "real in a different way," thus refers to the allocation of various experiences to different conceptual fields, on the basis of their invariant properties. What is regarded, then, as "unreal" is simply an object which belongs to a different conceptual field and has a different logical index from one which is, at the moment, being used for reality testing. The "unreal" elements comprise the variable features in any experience, the multiple irrelevancies which fill out the content of perception. But if the conceptual reference is altered, previously irrelevant features become the invariants of another type of reality. For example, if measurements are of primary interest, the invariant elements are those which may be conceptualized in terms of length, breadth, and weight. Color, content, age, use, and so

forth, are irrelevant. Under these circumstances, reality testing is based on certain numerical standards. The relevant conceptual standard may be altered when an artist sees the world in terms of form and color. Any phenomenon may be called "real" if its basic conditions are defined; and experience is "real" or "objective" when we understand how each element is located in the whole field of reality sense and reality testing.

There are many other bipolar terms which have been used to distinguish between the real and the unreal. Examples include the contrast between internal experiences and external events, between thoughts and things, phantasies and facts, wishes and actions, mental and physical occurrences, and, finally, between the ego and its object. All of these various oppositions depend upon the basic distinction between the experience of the self and that of the outside world. Self-experience is truly the sense of our own uniqueness and continuity, the feeling that we are able to control our actions, and the capacity for having unshared experiences, including all those so-called products of thinking, wishing, dreaming, and judging, which are *felt* to be within the mind. In the sense of William James, it is the "consciousness of self," the "I" which acts, rather than passively reacts, which is the experience of the self. In contrast, the outside world is everything which is neither me nor mine; it is an alien, autonomous sphere in which experiences are shared, where there are disappointments, surprises, and irregularities to our expectations and wishes. In psychoanalytic terms, the antinomy between the self and the world is represented by the pleasure principle and the reality principle. The apparent opposition between them is due to the inevitable dualistic character of thought, for the same principle underlies both aspects of the bipolar concept. The reality principle, in effect, restates the conditions by which instinctual impulses find fulfillment, and indicates how reality tests constantly reassess the field of the libido.

All interpretations of reality and unreality, hence, devolve upon the three basic bipolar concepts of the subjective and objective, the self and the world, and the pleasure principle and the reality principle. In turn, these contrasting concepts find a common denominator in the dualism between play and reality. By this fundamental distinction, mankind delineates the sphere of the "real" from that of "make-believe." It differentiates between the "pretend" world we carry about and the "real" world which surrounds us. Even here, absolute opposition between play and reality is only apparent, for when a child plays, he practices at dealing with the world. In altered forms, he rehearses reality, and learns to cope with it by developing his own sense of control and satisfaction. Play is a transition zone between autoerotic, aimless dreams and active participation in impersonal social and physical systems. There is both an exercise of ego control and an adaptation to phantasied, prearranged stress. Various objects may be indifferently altered to fulfill the play phantasy or game. Trees are castles; tables are ships; and puppy dogs are jungle beasts. The stress is pleasurable, and always under absolute control. The phantasy may be repeated, reversed, or terminated, according to the whim of the player; it does not necessarily sweep him onward in the flow of time. Games are played according to rules which must correspond in every detail with what has been anticipated. This is necessary because games and play deal with orderly and controlled events, instead of with the unpredictable, disturbing accidents which occur in "reality." Indeed, play, with its sense of absolute control and voluntary participation, ceases when countercontrol asserts itself. When the child is burned, matches are no longer playthings. Reality begins when the child has experiences for which he is not prepared, and which do not conform to his phantasies or predictions. So, too, for the adult who encounters reality face to face in the form of frustrated wishes and persistent discrepancies between events and anticipation of them. Experience defines reality by the difference between imagined events and the details of their realization.

Children play at stressful events taken over from an imagined adult world; the adult, by seeking to control events and to

glean pleasure out of the harsh and neutral circumstances of everyday life, attempts to find in reality the fulfillment that the child finds in games. Even as we move among others who both share and oppose our dreams, there is a lifelong pursuit of subjective meaning which is akin to the play distortions of childhood.

Play and reality may be further differentiated on the basis of reality testing and reality sense. The range of meaning in play is restricted to the libidinal field of one person who must regulate his actions in terms of relatively few rules and countercontrols. Reality provides an endless assortment of conceptual schemes, while there are only a few for the world of play. The more regulation and countercontrol which the person must undergo, the less prominent is his libidinal field in the total scheme. Play provides maximum libidinal opportunities and minimum conceptual restrictions. In contrast, the manifold meanings and unlimited conceptual indexes of reality reduce the scope of each person's libidinal field. For example, in a drama, the narrow conceptual field of play falls within the larger scope of reality patterns. Why do we not believe that the actor, *in reality* (that is, from every point of view), has committed murder on the stage? Although members of the audience may react to the event with conviction, by hating, crying, or fearing, in a token identification with the reality sense of the characters in the play, their active reality testing is able to differentiate the *play* of murder from the *fact* of murder. Reality testing recognizes the drama as a fusion of play and art, in which stress is deliberately contrived, and which may be voluntarily mastered, repeated, or reversed. Even when the drama depicts the most tragic and stirring events, the sense of control has not, in fact, yielded to countercontrol. Reality testing is able to index the staged events and the ensuing emotions into a specific conceptual field, which states, in effect, that criminals do not run around loose in theaters to commit murders by prearrangement. Within this range of meaning, stage actions are real in the domain of artistic phantasy, but are unreal as crime. Great artists may impose their creative phantasies upon the world with such conviction that reality testing is temporarily suspended, and, as a result, sweeping truths may challenge and change us. That there may be identity of emotional response in art between the creator and the observer is an observation at least as old as Aristotle's *Poetics*. At such moments, only reality testing is able to prevent the complete fusion of reality and art.

Not only philosophers debate the nature of reality; these questions are inescapable components of living. When a child or adult asks, "Is this real?" he has several unspoken criteria by which he evaluates events. First of all, there must be an initial capacity for forming conceptual models and for making judgments, in the same way that sight requires an intact visual system. In addition, the experience must either belong to the field of perception, or be ultimately related to perception. Beyond the primary considerations, however, criteria for "reality" demand that experiences yield certain invariant features with respect to the fixed relations of accepted ideas. But invariance itself is not reality; invariant features of events merely permit their conditional assignment to certain categories. Invariance represents a scanning of experience, in the present from the past and into the predicted future, and so applies to play as well as to reality. Furthermore, it is on the basis of invariants that propositions, hypotheses, and judgments are formed. The criteria of their validity or truth depend upon the accepted logical and symbolic systems of reference, which, of course, may vary from person to person.

Among the most common invariants of reality are certain general predispositions of thought, such as substance, cause, and free will, whose psychological origins explain why belief in them has survived all criticism. The idea of a single substance, or "principle" (depending on the idiom of the thinker), which underlies all phenomena, and which provides uniform material for the world, cannot occur without belief in reality itself. Indeed, belief in an "ultimate" truth or reality, however disguised, is a persistent human conviction shared by skeptic and

dogmatist alike. The belief in reality depends, in turn, upon the origin of the believing process, upon the objects which are believed, and upon the conviction that what we believe has universal application. The factors of "how we believe" and "what we believe" must combine with faith in the believing process itself—only then can we take action on our convictions without recourse to reality testing. Perhaps the concept of "substance," an absolute reality beyond experience, is an idealized projection of our own uncertainties and dissatisfactions. Reality sense may induce us to stretch out our intellects, to conjecture about the ultimate structure of the world, and to seek the "invariant invariant."

The concept of "cause" is another predisposition of thought which has survived generations of empirical and rational criticism. Persistent belief in causality is based on a naive acceptance which relates individual events to each other on a one to one basis. Theories of probability, which relate sets of phenomena, are quite different emotionally from this simple faith. Naive causality is expressed in the belief that one event will follow another due to an intrinsic relationship between the two, and that the first event determines the occurrence and characteristics of the second. The psychological prototype of causality is the act of "will," where a certain wish is immediately followed by an appropriate action. The wish, as Freud demonstrated, is at the center of most libidinal fields. Little wonder, then, that despite intellectual mistrust, man still looks for the immediate correspondence of wish and fact in his work and play. Yet the perception of events over which there is no voluntary control is an early, disturbing experience which leads to doubts of the efficacy of the will. Countercontrol forces us to recognize that will and wish are not omnipotent, that there is a hopeless, unfathomed mystery in the world, and that knowledge and fulfillment are always accompanied by despair. Faith in causality is an extension of the sense of personal control into the realm of physical events. One of the components of "belief in reality," it has already been noted, is the conviction that our beliefs have universal application. Thus, the persistent struggle between control and countercontrol, between wish and frustration, between phantasy and reality, is again exemplified in the ancient question of "free will" and "fate." The problem of 'free will' is really the problem of, "How much can we control?"

Given primitive belief in substance and causality on the basis of a reality sense for the ultimate relationship of things, the constant struggles and compromises between control and countercontrol stimulate us to understand the deeper structure of life. The search for invariance in the world is allied with the quest for certainty within ourselves. Invariance is a goal of reality testing, while certainty is a result of the fixed intuitions of reality sense. Together, they provide incentive for the investigation of nature.

SUMMARY

This paper maintains that reality and unreality are not absolute properties of certain events, but that any experience is real or unreal depending upon its conceptual index and libidinal significance. The "genesis of the belief in reality" actually presupposes what it sets out to investigate, for it is based on the supposition that there is an absolute, indisputable distinction between reality and unreality. A more fundamental approach would be to determine how certain experiences acquire a high reality value. One component of this process is *reality testing*, which discovers the invariant properties of experience. Through *perception*, *conception*, *symbolization*, and *logical systems*, bare sensorimotor experience becomes highly structured, differentiated experience, capable of having its meaning rationally defined. The other component is *reality sense*, which intuitively appreciates the nature, direction, and purpose of events, in accordance with their libidinal claims.

The source material of reality tests consists of basic convictions, called *acceptances*. These are our explicit and implicit beliefs which, although often wholly unproved, we never contradict. The method by which the range of meaning of an object is determined is *definition*, which consists of processes of

designation, differentiation, identification, and *implementation.*

Reality testing gives meaning to experience; reality sense provides for the intensity of experience. *Reality value* means that a man so believes in a certain reality that not only does he allocate it in a wider symbolic system and make predictions about it, but he is willing to take action, even to stake his very life, upon it.

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(Manuscript received July 22, 1957.)

In this article is presented a brief review of the work in machine translation of languages, with the conclusion that much more research needs to be done and that it does not appear that human beings will ever be relieved of the necessity of learning languages and of knowing how to use these languages effectively.

THE CURRENT STATUS OF LANGUAGE AUTOMATION[1]

by Gordon E. Peterson and Herbert H. Paper

University of Michigan

TODAY the development of complex automatic control systems is an accomplished fact. The techniques of feedback and servo-system design are well developed and have found many important applications. The time has come when instruments are being developed to perform many complex operations previously requiring a human operator, and interests in automation are developing rapidly. We have learned that machines can do, sometimes even better or more reliably, some of the things that man has been doing with his hands.

But can machines do what man has done with his mind? Here again, we have found that computers can do, sometimes better, or more reliably, and certainly faster, some of the things which man has previously done with his brain. Can computers be designed and programmed to learn some of the complicated things that man can learn? and to solve some of the complicated problems which he can solve? We now believe that they can, but we are only at the threshold of the development of systems which can simulate the more complex operations of the human mind—at the threshold of the development of automata in the broad sense of the term.

Electronic brains must have inputs and outputs suited to the environment in which they work; they must have some equivalent of man's sensory and motor systems; they must be able to deal with the delicate and refined variations in optical and acoustical patterns to which man can respond.

But most important and most difficult of all, these systems must be able to deal with language—not merely with simple, discrete codes, but with the enormously complex code known as human language. For automata must operate and serve within human cultures where both spoken and written languages dominate much of man's existence. The cultural and technological development which we know and take for granted is inconceivable without a long heritage of written records. It would be difficult to estimate the amount of our national effort which is devoted to the transmission and storage of spoken and written language (libraries, secretaries, telegraph, printing, telephone, recording, etc.). Even in those cultures that are primitive from a technological point of view we find highly developed systems of spoken language, all of which are amenable to being converted to written forms. And following the work of Benjamin Lee Whorf (1, 8) and others, there is some evidence that language in part dominates our beliefs, our attitudes, and our thought processes.

This report is primarily concerned with the nature of the language processes and with the problems involved in human language automation.

In order to identify these various processes, let us examine Figure 1. At the center of this figure is shown a very crude schematic of the primary sensory and motor mechanisms involved in language for one individual (individual number 2). At the

[1] This article is based on a paper presented at a Symposium on *Industry and the Human Being in an Automatized World*, May 24, 1957, at the University of Michigan.

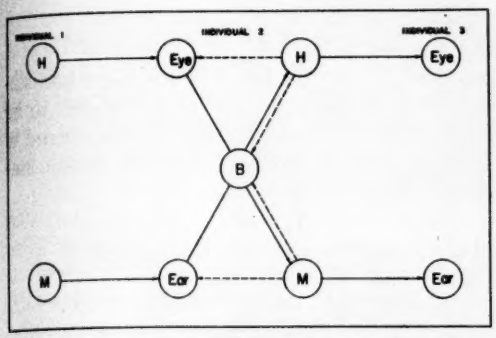

FIG. 1. A Communications Diagram for Three Individuals.
E—Eye and Ear H—Hand
B—Brain M—Mouth

left the two *E*'s stand for eye and ear, *B* is for brain, and the *H* and *M* are for the motor mechanisms, hand and mouth. What one says, he also hears, of course, and what he does with his hands he may see. These feedback paths are shown by dashed lines in the figure; they are of very great importance in human behavior, including language production. The importance of the oral-aural feedback path has been illustrated by having an individual speak into a magnetic tape recorder while wearing earphones which return his speech to his ears with the delay of the record-reproduce process. The result is often a violent disturbance of the speech production. It has been shown that this disturbance on the average is at a maximum in the range of 0.2 second (5), which is obviously related to the delay time around the closed loop through the Brain.

At the left in the figure are indicated the motor mechanisms of primary interest in communication for individual number 1, and at the right of the figure are indicated the sensory mechanisms of primary interest for individual number 3. We may consider individual number 1 as the initiator of the message and individual number 3 as the recipient of the message.

Individual number 2 can perform some very interesting functions when interposed in the communication path as shown in the diagram. If individual number 1 initiates the message in the spoken form, individual number 2 may repeat it in the spoken form; more importantly, he may transform the speech signal, which is composed essentially of continuous parameters into an orthographic (or printed) discrete code. In a similar manner, he may copy an orthographic signal created by individual number 1, or he may transform it into continuous speech.

Machines which could perform these two language transformations (speech-to-orthography and orthography-to-speech) would have many important applications. Extensive research is now in progress on the basic problems involved in each of these transformations and various preliminary instruments have been constructed. It does not seem unreasonable to anticipate that practical devices may be developed within the next one or two decades.

Let us consider the basic nature of the pathways indicated in Figure 1. It is obvious that they differ tremendously in nature and complexity. It was not until Shannon developed a fundamental theory of information (7, 12) that we had a means of comparing the capacity of these channels. In this theory, information about a system is defined in terms of the number of possible physical states of the system. The quantity of information involved in the occurrence of some one state, as the appearance of a specific symbol within a given code, is defined in terms of the number of binary choices necessary to select the symbol.

Under uniform conditions of transmission and reception, an efficient code is one having all states equally probable and independent of each other in occurrence. None of the components of speech, however, are equally probable (2). The wave forms, the individual speech sounds, the syllables and words all have certain individual frequencies of occurrence and they are influenced by what precedes and what follows. The difference between the amount of information transmitted if all conditions of the code were equally probable and the information transmitted when the code has more complex statistical properties may be defined as the redundancy. There are many ways in which to illustrate this redundancy; the method we have chosen involves an electronic gate which turns the speech off 50% of the time. This process is illustrated in Figure 2. In

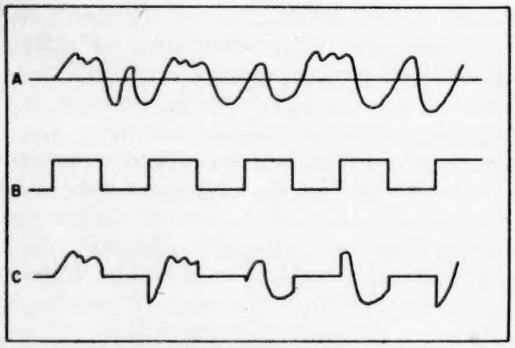

FIG. 2. Interrupted Speech.
A. Continuous speech wave
B. Gate function
C. Interrupted speech wave

the top pattern is shown a continuous speech signal; in the second pattern is shown the gate function; and in the third pattern is shown the interrupted speech.

In speech with an interruption rate of 50 times per second the speech is on just one-half of each period. This speech is not 50% intelligible, however, but is easily understood. In tests with isolated monosyllables, subjects can understand over 90% of the words under these conditions of switching (10). The intelligibility varies, of course, with the switching rate; only at extremely low rates, however, where approximately one-half of the individual words are heard, does the intelligibility become as low as 50%. Our knowledge of the language often makes it possible to identify the words correctly, even though only parts of them are heard.

Many other illustrations of redundancy in languages might be given. In general, it seems that human language is a code which operates with a redundancy of 50% or greater (8). It must not be concluded from this fact, however, that human languages are inefficient. We might better conclude that large amounts of redundancy are essential to effective communication under the great variety of complex conditions to which human language is subjected. It is not improbable that human languages involve a practical balance between needed redundancy and the desire for minimum time and effort in communication under relatively favorable conditions.

The linguist finds that he can analyze human language into an interrelated set of basic components on various levels. Every human language has a unique set of these basic components. The relationships of these components within each language are often exceedingly complex.

Let us start with an acoustical analysis of speech into its various properties. The analyses of the sentence shown in Figure 3 were made with a sound spectrograph which is the signal analyzer now commonly used in the study of speech. The spectrogram at the bottom of the figure (*D*) is constructed with a narrow analyzing filter so that the individual harmonics of the voice are displayed. The trace of the tenth harmonic shows the pattern of the fundamental voice frequency. It is the graph of this frequency which primarily depicts our perception of vocal pitch. The next, somewhat more complicated, pattern of random striations, gaps, and heavy black bands (*C*) is a sound spectrogram with a vertical scale of frequency from zero to approximately 3,500 cycles per second. Time is shown along the horizontal axis for each of the four patterns, and in the figure it extends for about two seconds. This spectrum analysis primarily reflects the articulatory movements for the vowels and consonants of spoken English. The third pattern from the bottom (*B*), on the same time scale, shows an over-all amplitude trace of the speech. The vertical scale here is approximately linear; so that this pattern is essentially what one would see on a cathode-ray oscilloscope with a slow sweep and the lower half of the wave inverted and superimposed on the upper half. The top pattern (*A*) is a similar amplitude representation from a probe tube microphone placed in the nose as the sentence was spoken.

It will be noted at once that there are essentially no discrete acoustical sounds in speech. The continuously changing characteristics of most of the patterns in Figure 3 are very conspicuous. In order to indicate further the continuous nature of speech, Figure 4 was prepared. In this figure are shown broad band sound spectrograms, along with corresponding traces from narrow

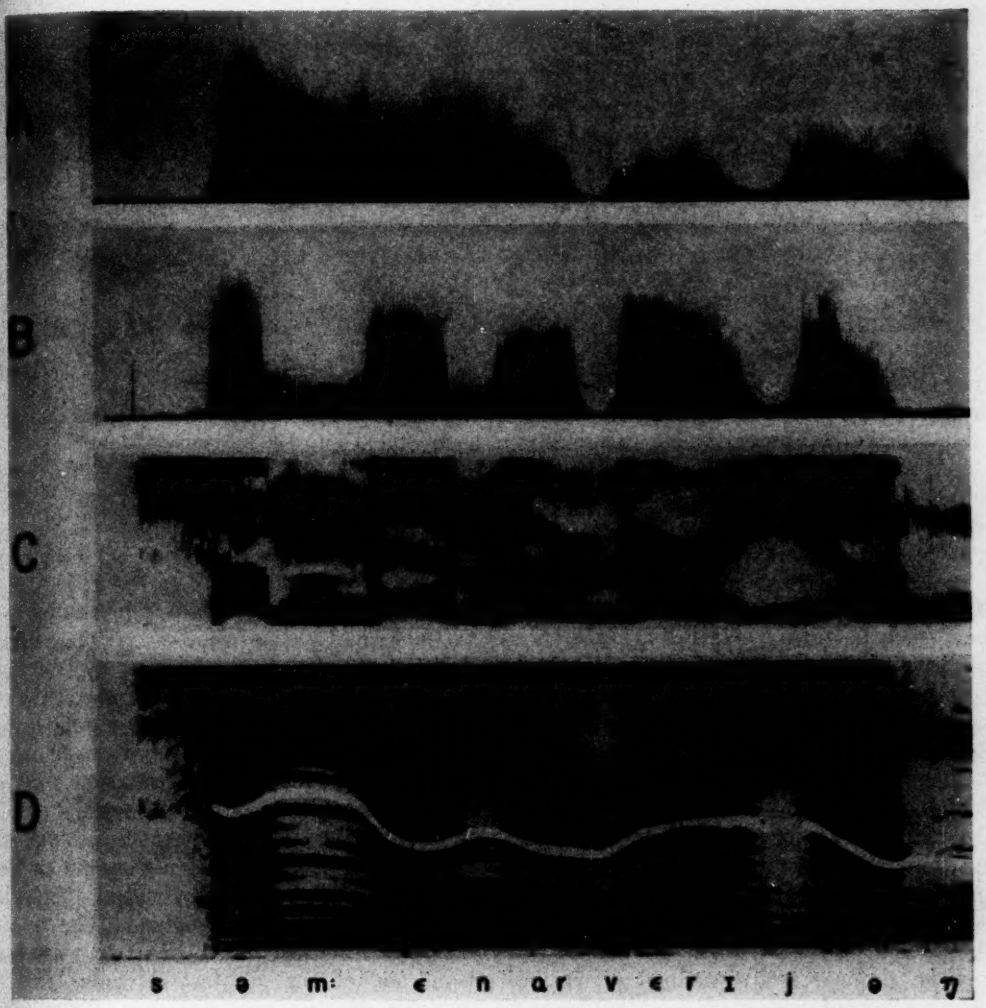

FIG. 3. Spectrographic Analysis of the Sentence: "Some men are very young."
Patterns from above downward:
continuous aplitude display from a nasal probe
continuous amplitude display from an oral microphone
broad band sound spectrogram
fundamental voice frequency

band sound spectrograms for the fundamental voice frequency. In the upper spectrogram, analysis of a normal utterance is shown with time along the horizontal axis and frequency on the vertical axis. For the lower spectrogram, discrete sounds which represented the isolated elements of the continuous speech were recorded individually. As the speaker produced the isolated segments he listened to the normal utterance and attempted to imitate the phonetic qualities and the vocal pitches which he heard. The isolated elements were then connected together to form a continuous tape.

When we compare the dynamic nature of the upper pattern with the broken or blocked-like patterns in the lower figure, it is not surprising that the synthesized utterance is not highly intelligible.

Speech synthesizers have been constructed, however, which make it possible

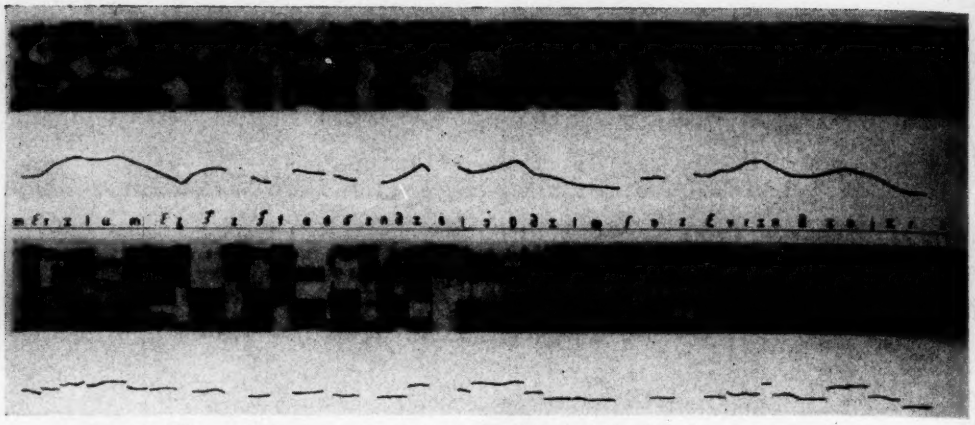

FIG. 4. The continuous nature of speech illustrated with the sentence: "Mary Lou measures her father's nice long eyelashes every nine years."
Upper spectrogram: Broad band sound spectrogram of normal speech.
Upper trace: Fundamental voice frequency of the same utterance.
Lower spectrogram: Broad band sound spectrogram of synthesized speech.
Lower trace: Fundamental voice frequency of the synthesized speech.

to insert the dynamic or moving aspects of the speech pattern. Such a synthesizer was constructed by Lawrence[2] which produces very intelligible speech. This machine operates from signals prepared on a glass slide. Six parameters are employed: the recurrence frequency of an input pulse, the amplitude of the input pulse, the frequencies of the first three formants (speech resonances), and the amplitude of a random wave. These six control signals are represented by six parallel time functions on the glass slide. They may be constructed arbitrarily, or may be derived from tracings of various types of sound spectrograms. These parameters make it possible to construct dynamic speech in which the various vowels and consonants are rather clearly produced according to various combinations of the parameters. When proper caution is employed in relating the machine output to human speech, such a device may be employed as a research tool in the study of speech, as well as a preliminary development with practical possibilities.

In listening to continuous speech, the linguist is able to analyze it into a complicated discrete code (7). A suggestion of how the variables of Figure 3 contribute to this code is given in Figure 5. In this figure three levels of oral language structure are indicated. At the left are represented the physiological mechanisms of speech, as previously referred to in Figure 1. The fact that the deaf can lipread, or that we often understand better when watching the speaker's face is ample evidence that a listener may derive information from a direct observation of the positions and movements of the vocal apparatus as well as from its acoustical transformation. In the center of Figure 5 are indicated the basic linguistic elements of language. The recognition of the nature of these elements has been one of the greatest advances in the analysis of language structure. They may be defined as mutually contrastive classes of speech signals. Within a given class the individual signals do not contrast. The individual occurrences of these signals are affected by what precedes and by what follows, however, so that the elements of the class are environmentally conditioned. The signals are classified differently, of course, in different languages. The basic elements are something popularly called "speech sounds;" in the terminology of the linguist they are "phonemes."

A phoneme may be considered as a class of signals within some time interval which is contrasted in human judgment with other signals over an interval of similar duration (9).

It should be noted that the phoneme

[2] Signals Research and Development Establishment, Christchurch, England.

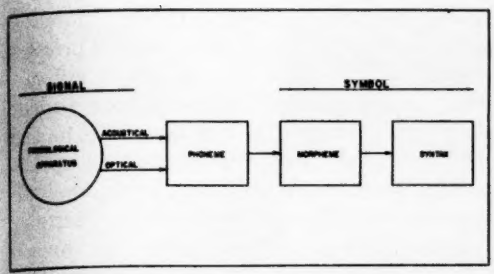

FIG. 5. The Basic Structure of Spoken Language.

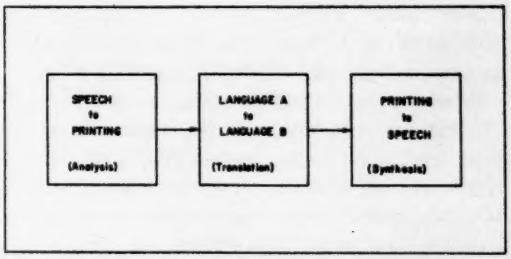

FIG. 6. The Processes of Oral-Aural Mechanical Translation.

does not have meaning in the sense that words and sentences do. Morphology and syntax also involve mutually contrastive classes, but these more complex patterns have a semantic significance associated with them. Figure 5 should not be interpreted as showing a sequence from the production to the perception of speech. The components of phonemes and morphemes are as much characteristics of the encoder as of the decoder; otherwise no communication would occur. Rather the figure shows different properties of the code and degrees of generalization or abstraction. (In general, second-order relationships are not indicated in the figure.)

The acoustical signal is essentially continuous over short intervals of time. At the level of the phoneme we employ discrete notations. In the production and interpretation of speech the phonemes are combined to form the meaning-bearing symbols of language. Both the phonemes and the morphemes may be identified as classes of environmentally conditioned, non-contrastive elements. By environmentally conditioned, we mean primarily that the components are affected by other components preceding and following in time. By non-contrastive, we mean that functionally they serve the same purpose and elicit the same human reaction.

In the communications diagram of Figure 1, individual number 2 cannot only convert the form of the message, but he can also translate it into another language. He can also both translate and transform, so that a message which is initiated in either the spoken or the written form may be transmitted in a different language in either the spoken or written form.

Work in language translation by machine has thus far been concerned primarily with printed forms. A complete oral-aural translating machine from one language to another is much more formidable. The basic operations are indicated in Figure 6. Essentially, three highly complex processes would be involved; first, the conversion from spoken input to printed output; second, a mechanical translation from the printed output of step 1 (which now becomes the input for step 2) to a printed output in a different language; and third, the synthesis of speech from the printed form of the new language. We will leave to the imagination a time schedule for the development of such complete linguistic automata.

At the present time research is in progress by several groups throughout the United States and Europe on mechanical translation. There are three basic methods under investigation.

1. The first method is essentially empirical in nature. With this method a very limited text is translated from one language to another, and the detailed procedures for making the conversion are recorded. As new text is gradually added, the expressed procedures are modified and expanded accordingly. In this way one might expect eventually to arrive at a stage where it would be unnecessary to add further procedures for converting from one language to another.

2. The second general approach involves the use of an intermediate or special language of some type into which and from which all translations are made. This method would reduce the number of translation systems necessary when it is desired to have several languages mutually convertible.

3. The third general method involves more restricted materials. The theory which underlies this method considers human lan-

guage to be closely related to culture, in the sense of the anthropologist. Each language is considered to present a special cultural analysis of the physical world and of human relationships. In addition each language has its own structural form. Thus the mapping of one language into another is considered to be a very complicated process and direct matching of linguistic structures for any particular pair of languages is considered most effective. Relationships between the cultures and relationships between the two linguistic structures are studied and rules for the conversion of one to the other are derived. It should be noted that there is a special complexity to language translation—for not only must the two linguistic structures be described, but also the distribution or occurrence of each element (as a word) within the structure must be determined and stored so that it can be properly translated.

Let us *summarize* briefly: In the automatic conversion of speech to orthography, we are primarily concerned with the transformation of the relatively continuous parameters of speech to the discrete components of written language. The simplest transformation is the conversion from the speech signal to the basic discrete linguistic elements: phonemes and morphemes. Complex computing apparatus would be required to convert these to conventional orthography: letters, words, and punctuation marks. Thus far progress in automatic speech recognition and in machine translation is in a very preliminary stage. At present there have been approximately four different instruments constructed which are able to recognize a limited number of phonemes and syllables or words (4, 11, 6, 13).

Linguists are at present engaged in the laborious task of analyzing certain languages in very great detail so that computer programs may be prepared for their translation. In the process of learning to program machines for linguistic research, it is expected that much detailed information about language structures will be developed.

We may *conclude* that much theoretical development and basic research is in progress which will contribute to language automation. It appears that we may have to look very far into the future, however, to find automata that can follow complex spoken instructions. It does not appear that we will ever be relieved of the necessity of learning languages and of knowing—as human beings—how to use these languages effectively. We see at least no immediate threat that machines will be dictating the instructions for man to follow.

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(Manuscript received November 7, 1957.)

An adduction and elucidation relative to the problem, especially as it is involved in contemporary historical analysis of an interdisciplinary character, of the relationships among the scientist-observer and the thing-, or things-, observed as well as the interactions between the scientist-observer and that which is observed and made the bases of theory construction, reveals that the classical nontransactional and nonsituational conception of objectivity must be re-evaluated.

THE PROBLEM OF CONTEMPORARY ANALYSIS IN HISTORY AND PSYCHOLOGY[1]

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THEORY in psychology has, in the main, sought to maintain the classic ideal of observer-separation from the phenomena observed. Where observer-involvement did obtain, technique for procurement of data or for exercise of influence was its excuse; ultimate theory was to be cleansed of it. Thus, we have the technique of participation-observation in social psychology, transference onto an observer-therapist in clinical psychology, etc. But the theories which provide the rationale for these techniques posit, as a rule, observer-free data in line with the ideal of classical physics. Involvement of the observer here does not connote theoretical, but applied, psychology.

The experience, however, of the writers in social psychological research with strong interdisciplinary implications has left them skeptical of the *universal* need and utility of the classic ideal which separates in theory the observer and the observed. The writers propose, therefore, to re-examine the relationship of the observer and his data. Argument will be entered on behalf of the observer-in-theory and, even more radically, on behalf of that neglected "personage," the actor-in-theory.

The writers have been made particularly aware of the problems involved through encountering them in especially severe form in contemporary *historical* analysis—an ever-present feature of the interdisciplinary research pursued by the writers (24–33). For this reason and because the problems of contemporary analysis in *psychology* are so cluttered with side-issues, obscured by traditional treatments, and tied to one allegiance or another, it has seemed wise and economical of effort to present the argument in historical terms as if to historians, leaving it for the most part to the psychologist to see, *as he must*, the acute applicability of the argument to his less drastic situation. The familiar problem in unfamiliar context is thereby illuminated; it avoids likewise the ready distraction from essentials. Furthermore, since the problems of contemporary analysis are especially troublesome for the historian, a double purpose is served by this device of presentation (1, 5).

In the argument that follows there will be hardly a point that has not been previously made by some psychologist or historian in one connection or other. What is attempted here is only a workable synthesis, broadly contributing to a solution of the problem of contemporary analysis of phenomena where it is immediately important to note how the present is meshed with the past and the future is geared to both.

ARGUMENT

The world scene in a sense constitutes a laboratory—a laboratory of contemporary

[1] This paper reflects research undertaken with the support of the Inwood Institute and the Dearborn Foundation.

history, as it were, putting relevant theories and generalizations to the test and gauging the adequacy of their assumptions. On the other hand, history is a "bad" experimenter in this laboratory, in that its research designs are not neat, while the experimental conditions are such that it is frequently difficult to decide after an experiment just what has been proved or disproved, suggested or not suggested.

Direct or univocal conclusions in the contemporary situation are thus hard to manage. In lieu of these what have we? Generally, a spate of mediated conclusions, whose character and reliability are determined in part by the assumptions, stated and unstated, that operate in any logical or quasi-logical procedure and in part by the mediatory processes employed on the "experimental" data—data which come, therefore, to be variously discerned or even constituted, variously assembled, and variously handled by the human processor who is at the same time our aforementioned observer.

If this processor is simultaneously a major actor on the world scene, difficulties are multiplied, since the data which he manufactures through his own actions on the basis of *his* reading of events, as they involve him and his purposed activity, may turn out to be corroborative merely because he *believed* his theories and assumptions to be correct and *acted* accordingly. History often has a way of bequeathing this kind of retrospective validation to the line of action that turns out to be successful (17), and makes thereby a mockery of the "objective" test insofar as historical theory and analysis are concerned. In other words, where man significantly involves himself in history, a man-free, so-called objective analysis of events may not be even a desideratum since such analysis ignores man's constant intervention into events where they concern him and simultaneously overlooks the modifying reciprocity between *events* and man's interested *evaluation* of them (1).

The world scene as a laboratory of history presents yet another difficulty in that its staging is to a large extent ambiguous. As a matter of fact, its props frequently can be made differentially discernible only by the light of one or another theory, and sometimes it is as if they were of a theory's own manufacture.

Closely related to this difficulty is what one might call the particularism of theories: one theory of events yields an accounting of some aspects, stressing these; a second theory accounts for others, stressing those. And, as if to underscore the nature of the difficulty, the two may be both refused because of criteria set up by still a third theory![2] It is almost as if these aspects, separately discerned in theory and understood, were not simultaneously attributable to the same set of events, as if something like a principle of plurality of explanation, suggested by Bohr's principle of complementarity in quantum physics,[3] were in the permanent offing for historical science, in other words, as if no precise, unitary theory, covering all aspects of events, were *in principle* attainable (2, 7, 8, 11, 13, 16, 17, 23, 37).

The nonrepeatability of historical events, along with man's intervention into them as actor, complicates further the application and testing of theories and analyses, making verification in the classical sense operationally meaningless. In other words, where a *different* action or intervention, theoretically prescribed,[4] could have altered the course of events in presumably predicted

[2] A similar problem exists for the events themselves: one theory may notice and yield an accounting for one set of events; a second theory may notice and account for another; and both may be rejected because of criteria elaborated by still a third theory which notices these and other events differently and applies its own accounting thereto.

[3] In physics, in order to measure the coordinate of a material particle and its corresponding momentum (or velocity component), mutually exclusive processes have to be employed. For this reason the two concepts of position and velocity, although they have precise physical meanings separately, may not be attributable simultaneously to the same particle in a precise manner. This *necessary* logical relationship between the two concepts has been designated by Bohr as *complementarity* (35, p. 137).

[4] A theoretical analysis, when made the basis of action, is here thought as yielding a theoretical *prescription* of action rather than merely a prediction.

directions, ideally a test of theory could be had by a resetting of the past and a retesting, that is, by observing, collating, and checking the end results of the applied action alternatively conceived. But initial conditions on the historical scene cannot be reset in order to interpolate, on the basis of theoretical prescription, alternative action into the developing course of events. This difficulty, however, could be met in some measure, were it always possible to demonstrate the essential equivalence of outwardly disparate sets of events, separated in time. But, in view of both the emergent and divergent character of world events (13, 17, 23), the likelihood of such general demonstration for large-scale events, which often fracture the continuities of history in an erratic fashion, is about zero.

As previously suggested, the involvement of man in the historical process and his interest in his fate therein offer a fundamental difficulty for the testing of theories and analyses, at least, in the classic sense. It is here as if it mattered to individual electrons what the result of an experiment would be and they tried—to the dismay of the "objective"-minded experimenter—to influence the course of events in the physical domain. Unlike the situation in physics, however, historical theory cannot withstand the intrusion of man as actor in the historical process. Accordingly, the expected fruits of prediction are frequently denied to such theory, although, when the action of individual men was slight, ignorable, or irrelevantly directed, such theory may have enjoyed a fortuitous success in default of purposed contrary action which could have put reins on the gallop of events and diverted them from theoretically assigned outcomes (1).

This is the situation when action contravenes theory and leads to the latter's failure. What of the situation when action is undertaken on the basis of theory and, *for this reason*, should confirm it? Evidently a serious difficulty is involved with respect to a theory whose testing is crowned with "success" merely because this theory, as an implicit or explicit *theory of action*, had converts and, being a guide to the faithful, served to influence the course of history; in other words, when it is precisely the action one proposes to take on the basis of theory or analysis which helps promote its verification.

The preceding discussion would make it seem as if historical theory and analysis, "objectively" conceived and rendered, were a chimera. The person is, after all, in the very nature of things involved in the historical process, and it is hard to decide when his involvement as actor is crucial and when not; and, if crucial, how to include him in theory and at the same time stay open to "objective" verification in the laboratory of history.

As the various difficulties have been formulated, a resolution of them would appear unlikely. But it would seem that their attenuation and even disappearance were a possibility, provided that certain basic, though ordinarily unstated, assumptions, generating these difficulties, are made explicit and reviewed in the light of recent thinking in a number of the nonhistorical disciplines. The most important of these assumptions follow:

1. The historical scene is "objectively" existent, and it is the business of the historian to describe and explain it. (To call into question "objective existence" is not to indulge in solipsism, for there is no denial here of an external "existence." What is questioned is "objective existence" in the classic sense which affirms, contrary to relativity theory, that a *description* of whatever exists may always be had in terms of properties *independent of the observer*. For example, one ordinarily thinks of the length of a ruler as an inherent property of that ruler. Actually, however, although a ruler under observation does exist, its length is not an intrinsic property, but is partly a function of the observer's velocity according to the relativity formula: $L' = L\sqrt{1 - v^2/c^2}$ (15). In other words, "existence" is not synonymous with "objective existence," and to affirm the former is not necessarily to affirm the latter, since description of what exists may be significantly and necessarily observer-involved. Only when

observer-involvement is negligible may one speak of "objective existence.")

2. Events on the historical scene are completely determinate and, except for their enormous complexity and fleetingness, completely determinable.

3. The aim of theory is to describe, explain, and predict, and in one way or other be testable.

4. Man, on behalf of scientific theory, must remain outside it or be viewed, if he is to be invited inside, as merely a contributing or complicating factor—the additive one in the $n + 1$ variables of historical analysis and theory.

5. Analysis of the contemporary historical scene and that of the past are ideally the same.

If we are to judge from developments in theories as remote from each other as quantum mechanics (4, 35) and transactional psychology (3, 6, 12), each of these assumptions, though long uncontested, is eligible for re-examination. For example, in quantum physics the experiment is recognized as an essential interference in the order or disorder of things, so that resulting theory is a theory of *interferences*, as it were, rather than one of a so-called objective world whose composition, after interference effects have been "canceled out" or "adjusted for," can be pieced together mosaic-like from the data of experiments. Where interference effects are negligible, one may, of course, speak of an objective world; where not, the world, though objectively comprehended, may no longer be thought of as objective in itself, that is, as possessing properties independent of the observer, his measuring instruments, his methods of analysis and purposes.[5]

Or consider yet another instance—the assumption of the completely determinate nature of events. Classical physics had long established the idea of full determinability with sufficient knowledge of key antecedent conditions. Quantum physics, which came to replace classical physics because of the latter's failure to cope with crucial problems, asserts, however, the probabilistic nature of things and seeks to accustom man toward viewing the world as not completely determinable in the classical sense in spite of this so-called sufficient knowledge of pre-conditions (35).

Many events, to be sure, do follow a *convergent* course toward expectation, so that one may here speak of determinacy; others, on the contrary, increasingly *diverge* therefrom. And it is the latter which especially confound attempts to force events into a predictive straitjacket. It is not that the variables to be kept track of are too multitudinous and complex that theories and analyses in history may fail, but rather that it is in the nature of things always to have untoward, often mushrooming, effects from unsuspected sources, *retrospectively* important because the unanticipated happened, and to have things that are in themselves slight cause abrupt tangential detachments from the contemplated courses of historical development. These are matters which classically conceived theory cannot or will not confront (4, 7–11, 13, 16–23).

Let us grant that most events that are tentatively divergent do submit finally to the convergizing action of the historical matrix in which they move, thereby allowing the fulfillment of expectation. This, however, does not preclude the existence of other events that either in a near flash or subtly through time change the very matrix itself which *before* limited their divergence from expectation, but which *now* permits the display of convergizing tendencies that are new and allows thereby the entertainment of new expectations. The divergent thus becomes the emergent with the face of the latter, as novelty, only partly defined by the past out of which it seemingly took shape.

The customary damping, if not suppression, of divergence through convergizing action of the matrix in which divergent development must take place is neatly illustrated in genetics. A mutant gene in a dog, for example, usually does not give rise to a genetic end-product which is not recognizably an ordinary dog. Expectation here is fulfilled to a high degree through the convergizing action of the genetic matrix in which fetal development is organized. However, should the matrix itself be changed,

[5] See explanatory addendum to assumption one.

that is to say, should its organizational properties be altered in the process of elaborating the mutation, the resulting deviation from expectation may be so great as to permit us to speak of a significant divergence (23). In history an abortive revolt would illustrate the former contingency; an unexpected mob action, such as occurred in Poznan in 1956 with institution of the Gomulka regime as unforeseen consequence, would illustrate the latter.[6]

The assumption that analysis of the contemporary historical scene and that of the past are ideally the same, that what you would like to see, if you could, in the one analysis is what you would want to see in the other, is also challengeable. However "objectively" approached, analysis of the contemporary scene, whether by an actor in it or by a "disinterested" dissector of it, *is* different from analysis of the past, if only because the former suggests one or another theory of action which, when embarked upon, may drastically alter the course of history; whereas the latter attempts only a record and an understanding of what took place,[7] though even here, if contemporary relevance is discerned, a theory of action may be implied. Much that is unsatisfactory in contemporary analysis, certainly from the point of view of the history-maker, may be traced to failure to recognize this essential difference between analysis of the now and analysis of the past.

The view that man is a disinterested confirmer or rejecter of theories from which he must stand apart, while eminently applicable in the natural sciences and increasingly applied in the social and historical sciences, is challengeable where man, his goals, and destiny are concerned. False theory changes no planet's orbit, rearranges no lines on the broad band of a spectrum, but can alter the course of events on the world scene. Marxist theory finds no counterpart in Ptolemaic theory, for evangelical belief in the former changed the very phenomena under theoretical purview; dogmatic belief in the latter certainly did not.

For the classical physicist theory provided a description of an objective world independent of the observer, his laboratory apparatus, and the information obtained thereby. But in relativity physics description cannot be provided without involving the observer, since relative velocity is integral to this description (15). In quantum physics the process of disengagement from classical belief goes even further, for here the very phenomena theorized about are also a function of the apparatus used to elicit them (35)! Where then is the objective world of the classicist? The world is, of course, still there, but it is no longer equated with the theorist's description of it which necessarily includes man, the observer, and the irreducible paraphernalia of his laboratory.[8]

Furthermore, in the natural sciences information gained and the purposes of its elicitation exert no reciprocal effects on what was observed or experimented upon. However, in the social, psychological, and historical sciences, instances abound where precisely this does occur. If knowledge, purpose, and ultimate concerns do affect in varying degrees what is to be theoretically comprehended and do influence the course of events, how can we decry their inclusion in theory as an encroachment on objective explanation and, therefore, as elements that must somehow or other be ingeniously bypassed? Are these perhaps not fundamentals as

[6] Jordan has termed the production of large-scale effects, "amplified" from slight erratic beginnings, as *Verstärkung* (7–10); Langmuir applies the term *divergence* to this situation (13). Conceptually *Verstärkung* is more satisfactory than Langmuirian divergence since it implies divergence within an organizing matrix which may or may not be convergizing in the original sense of its action. The latter contingency would obtain if the matrix itself should be modified by a divergence and exhibit a new convergizing action, replacing that of the original matrix (23).

[7] The record is certainly not a facsimile of things transpired. Many events are retrospectively singled out for historical attention merely because subsequent happenings rendered them important (17); others are attended to because scholarly fashion ascribes to them a significance of some kind; and so on. As a matter of fact, "profundity" in historical analysis is frequently merely an exercise to demonstrate one's ingenuity in ferreting out such events and contriving the quasi-logical concatenations necessary to provide the appearance of inevitability (1).

[8] See explanatory addendum to assumption one.

necessary to the proper theory of man as are the velocities and measuring instruments of observers to physical theory? Can they be bypassed? Should they be bypassed merely because classical conceptions of so-called objectivity are violated?

Again, what is the historical scene? A setting in which actors like puppets are deployed in manipulative fashion? Must we believe that this is the objective reality which the analyst is to comprehend and explain? In other words, does such really exist, or do we have to acknowledge, as the previous discussion would suggest, *many* realities shaped by the actors themselves, whose worlds have been and are being designed and fashioned by the experience of the past, the aim of the future, and the amalgam of the two in present action and establishment of individual purpose? That this is not bizarre speculation, but well-founded possibility, is attested by the work of the transactionalists in psychology who in their experiments in visual perception have come up with findings whose analogues, if not corroborative, at least are extremely suggestive (3, 6, 12).

In other words, action and course of action must run off in the actor's world and not in the analyzing observer's. Otherwise, action which is natural for the actor may be enigmatic for the observer. If he is to avoid the latter, all too common, condition, it is axiomatic that the observer must understand how the actor structures his world and must simultaneously avoid those perceptual propensities of his own which lead him to articulate the data of world events into explanatory patterns fashioned from his own experiential templates.

Actually, to gauge the actor's view of the scene in which his actions are staged requires that one attend to at least five elements: his *purposes*, both large and small, remote and near, the *situation*,[9] in which his purposed *actions* are undertaken, evolved, and frustrated or consummated, and the relevant *experiences* of his personal past which allow one to assess the weighted *probabilities* he assigns to outcomes predicated on actions to be undertaken. From behavior in which at least these five elements are noted one may re-create for analysis the actor's structured world.

It follows, then, that behavior, whose analysis does not exhibit an integral incorporation of situation and purposed action, genetically illuminated, must be thought of as inadequately comprehended, however brilliant the analysis may be in terms of the categories and artifacts of conventional analysis. In other words, at the very least, behavior on the historical scene must be analyzed situationally; prognoses must be projected situationally. Purposed action is always situationally involved; further, it is sensitively adjustable as the situation itself may yield to noticed or felt change by such action.

In this connection the converse is also worth noting, namely, the situation itself is to be understood, at least in part, in terms of the spectra of possible actions and purposes which it potentializes and which in a real sense help define it. In other words, a situation in history is not merely a field for the play, interplay, and counterplay of various forces and factors or the mere occasion for the exhibition of properties and characteristics. The situation is warp and woof of all purposed action undertaken and, even if it does not fully set the design of the latter, certainly determines its texture.[10]

PRACTICAL APPLICATIONS

The two related examples which follow are adduced to illustrate a number of the foregoing considerations.

[9] Situation here must not be thought of as a variable X to be appropriately substituted in some equation. This is a habit of mind developed over the years by an overly process-minded psychology which assigns to content a trivial role in theory. *What the situation is* is just as important as the fact that the situation is a relevant "variable." It is not merely an X waiting for appropriate substitution in some equation; it changes the equation itself. Purpose likewise is to be viewed as more than a variable in some process equation. *What the purpose is* has effects transcending mere substitution for an X. For a sympathetic treatment of related views, see McClelland's article on content-psychology (34).

[10] The role of content as an integrator has been overlooked in the social sciences where the tendency is to blame an "inadequate conceptualization" for failure to integrate.

(*A*) How useful is a concept such as disloyalty potential of a population? Actually, the concept is operationally difficult to handle, since it cannot be gauged prior to an actual manifestation of mass disloyalty, whereupon a *post mortem*-like judgment of its previous degree may be gained. An operational comprehension of the concept may, for example, fix upon a numerical index such as frequency of local signs of disloyalty or disaffection in a population over a period of time. This index may register high. Yet, when the chips are down and the populace as a whole must make a choice, mass manifestation of disloyal behavior on the part of a populace whose disloyalty potential was considered high may be near zero. In this case one would have every right by current practice to impute, contrariwise, a *loyalty* potential of high degree to the populace. As a matter of fact, the Soviet populace exhibited both mass "loyal" and disloyal *behavior*, the former after the latter, within the space of the first few months of the German attack on the Soviet Union,[11] from which one might infer the untenable prior existence of two potentials, each high and of opposite sign.[12]

Why the paradox? Primarily because the concept of disloyalty potential has not been situationally referred.[13] The concept fails when stated apart from the situational matrices in which a people, exhibiting such a potential, may find itself. This, in turn, requires one to consider what the people as a whole may see in the several situations and how for any given situation this people may choose its course of action from a set of choices in order to realize its larger aims—aims which admittedly may even change or transform in the pursuit of fulfillment.

The transactional approach to the concept of disloyalty potential requires ascertainment of a people's aspirations and habits of purposed action, the spectrum of possible situations bringing these into play, and a corresponding spectrum of disloyalty potentials, whose indices are estimated on the basis of how the people probably perceive these situations as affecting it in fundamental ways and as offering possibilities for a variety of purposed actions.

If spectra, as proposed above, should be taken into serious account in the determination and implementation of a course of action, a wider range and richer choice of plan becomes possible, because the actor in history need not limit himself to situations wished upon one, as it were, but may bank on deliberately engendered situations, alert at the same time to the divergent possibilities, positive and negative, inherent in such action. In other words, disloyalty potential does not reside in a people alone, but is involved also in the situation in which this people may find itself. And situations may be created as well as merely encountered.

(*B*) Similarly, one may question the tenability of judgments concerning the "general" reliability of Soviet troops based merely on noting the generally "reliable" behavior of these troops during the recent Soviet intervention in Hungary. Such judgments mirror the world of the analyzing observer, but overlook that of the actor—the Soviet military man. The world in which the latter finds himself is, after all, what *he* perceives it to be, not what the former sees. Accordingly, purposed action which is nontransactionally and nonsituationally observed and analyzed cannot be utilized to yield ratings concerning reliability of Soviet troop behavior in future situations, because effective clues as to the probable structuring by Soviet troops of these situations in a time of future stress and decision are not to be found thus.

Why should we have expected Soviet troops, even if of anticommunist disposition, to have acted in Hungary other than the

[11] Actually the mass "loyal" behavior which followed initial mass defection cannot be taken as an indicator of real loyalty, since here we clearly are dealing with its form and not its substance, in other words, "external loyalty." For a measurable time there was in this case *for the regime and for the people* a convergence of immediate purposes—for the regime self-preservation, for the people defense of self and the motherland—with realization of the one entailing realization of the other.

[12] To speak here of an ambivalence of potential is only a semantic dodge which solves nothing.

[13] In the beginning the Soviet populace viewed the Germans as liberators from communism; later, German intentions were seen as involving the destruction of the Russian people and nation.

way they did? Why should we have been surprised to discover the small number of defectors to the West from among the mass of army anti-regimists?[14] In great part the answer lies in failure to assess the situation as the Soviet troops saw it. Did they see themselves in a struggle pitched *for them* in meaningfully political or ideological terms?[15] Evidence indicates they did not. They for the most part saw an uprising which became increasingly anti-*Russian* and increasingly a matter of sheer kill-or-be-killed action (36, 38). Since these are fundamental considerations for any man and patriot, Soviet troop behavior in Hungary provides poor clues as to behavior in future situations other than that we may expect more of the same if the latter should be construed as encounters fraught with harm to the homeland or as a fight for mere personal survival.

Failure to appreciate how various considerations, such as the above, may make a predominantly anticommunist mass fight *for* the Soviet regime can be disastrous. Ratings of Soviet troop reliability that are nonsituationally and nontransactionally developed are, therefore, to be especially subject to review, since reliance on them can bring about untoward intervening effects into the historical scene as the latter evolves into the future.[16]

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(Manuscript received August 26, 1957.)

Scientists are not dependent on the ideas of a single man, but on the combined wisdom of thousands of men, all thinking the same problem, and each doing his little bit to add to the great structure of knowledge which is gradually being erected.

—LORD RUTHERFORD

No matter what their specialty, from hair styling to sheep genetics, the authorities in every field nowadays seem to regard it as obligatory to convene in a sacred grove once a year, divide into as many panels as possible to insure a maximum of obfuscation, enshroud each other in verbal cobwebs, and, finally, issue to the press a conclusion any newspaper reader could have reached without leaving his bathtub.

—S. J. PERELMAN, *The Road to Miltown.*

Once upon a time the expert in international relations did not need many skills beyond an ability to speak with suavity and distinction in more than one language and to know a bit of the history and customs of a few countries besides his own. But times have changed and now the student of international relations should be not only a gentleman and a scholar but also, of all things, a scientist.

THE PLACE OF BEHAVIORAL SCIENCES IN GRADUATE TRAINING IN INTERNATIONAL RELATIONS

by Karl W. Deutsch

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THERE are, broadly speaking, three types of judgment relevant for the study of international relations, and a graduate school specializing in that field might find it advisable to give some reasonably advanced training in all three. These three types of judgment are (1) experience (2) logic, and (3) behavioral research.

EXPERIENCE: JUDGMENTS FROM HISTORY AND DESCRIPTION

The first type of judgment is qualitative. It is based primarily on experience and description. What are relations with foreign countries like? What does experience reveal about them? How have statesmen and peoples acted in international politics? What have governments considered to be the national interests of their countries, and what have they done to preserve or attain them? How have the interests and actions of smaller groups or individuals affected the outcome?

If there is much time for such investigations they usually become the province of the historian; and the historian has the advantage of dealing with situations which had some definite outcome. Alternatively, they become the province of the descriptive political scientist who can deal with them in a thorough and scholarly manner.

If there is little time, however, and if some decision has to be made on the basis of limited information, the situation will call for something else. It will demand the rapid skills of the administrator, the statesman or politician, the lawyer or judge, the executive, and generally the man of affairs. In all these callings, men must be able to discern what is relevant in a case, what constitutes evidence and what does not, and how conflicting evidence should be weighed.

No matter how much or how little time we have, we must seek to approach three kinds of understanding: The first is cognitive understanding. Can we put ourselves imaginatively into the place of another person so that we think we know what he thinks he knows—that is to say, can we guess what his "map of the world" looks like to him? The second is emotional understanding. Can we in our imagination feel vicariously as he feels, value what he values, feel his inner tensions as if they were our own—regardless of whether we approve or disapprove of his purposes? This is the understanding of what the sociologist Talcott Parsons would call the "evaluative" and "cathectic" aspects of his actions. The third kind is situational understanding. How does a man's action fit into the context of the goals he seeks, and how do his goals, and his efforts to reach them, all fit into the larger context of the situation in which he finds himself? How can we understand his acts, feelings, and expressions as parts of some larger whole which adds to their meaning? Whether in painstaking research or in quick intuitive human grasp of other personalities, cultures, and situations, we must always try to approach as

best we can all three of these modes of understanding in international politics.

Universities can teach more easily the slow skill of the descriptive scholar than the quick skill of judging affairs. Indeed, they cannot teach directly this latter skill, since it is acquired, mainly, if at all, from the experience of life. They can simulate, however, some of this experience—and make it wider in some respects than the possible life experience of any one person—through teaching history, and further through the study of specific cases in that field, as well as of cases in diplomacy and international law. To a lesser but significant degree, any broad humanistic education and any broad acquaintance with literature, particularly with serious fiction relating to human behavior, seems likely to contribute to these skills.

For all these reasons, an emphasis on the history of international affairs and of diplomacy should always continue to have a prominent part in the advanced training for any career in international relations—even though it should be supplemented by at least some training in the two other methods of making judgments in international affairs.

LOGIC: JUDGMENTS FROM ABSTRACTION AND DEDUCTION

The second kind of judgments requires what we might call analytical and deductive skills. Here the emphasis is not on the rich flow of unique cases and situations, but on the search for significant resemblances and contrasts between them. Relatively general regularities are to be discovered by comparison. Singled out from their original contexts by analysis and abstraction, they are formulated as general principles from which consequences are deduced for application to other cases. Thus a principle of international law may be derived from a series of precedents, or a rule of foreign policy inferred from a series of diplomatic actions; and from these general rules the analyst may derive applications as to what should be considered most important in some other case, or what should be expected to happen, or what should be done.

Training in this type of thinking is gained particularly from the study of international law, which is thus relevant for the study of international relations even beyond its great substantive importance. Thinking of this type, however, is by no means limited to law. The study of international economics teaches not merely empirical data, but deals also to a large extent with a body of logically related abstractions, such as supply and demand, equilibrium and disequilibrium, comparative advantage, consumption functions, and the like. Finally, in the study of international politics, such concepts as the balance of power, the presence or absence of a "power vacuum," the concept of a vital "national interest," all have important analytical and deductive aspects.

Classical political science, as well as classical legal theory and classical economics, have all heavily relied on this abstract analytical and deductive approach. It will and should continue to be effectively represented in any advanced training program in international affairs.

However, even in combination, training in the first and second types of judgment can be supplemented to great advantage by a third approach so as to make up a well-balanced program more nearly adequate for the needs of the present and the foreseeable near future. For this purpose, competence in assessing a third type of evidence, and skill in making a third kind of judgment will be needed. This third kind of judgment, and the training needed for it, is related to the methods of the sciences, such as observation and experiment, including the quantitative treatment of empirical data. In its application to human affairs, this approach has met both with particular pitfalls and with particular opportunities; and in recent years, much of the work done in this direction has been comprised under the broad name of the "behavioral sciences."

BEHAVIORAL RESEARCH: JUDGMENTS FROM OBSERVATION AND EXPERIMENT

The approach of the behavioral sciences is that of the sciences in general, limited as well as enriched by the fact that they are dealing with the behavior of human beings.

The word "behavior" suggests that this

approach is primarily concerned with that which is observable by more than one observer, or which seems attested by more than one type of evidence—in short, with that which can be verified by methods valid for more than one person. An inner emotional experience of a statesman thus is not "behavior" in this sense, but his statements in an interview, or in a letter or diary, would be, and so would be any observable actions on his part. The behavioral approach distinguishes between external facts that can be observed and verified, and unobserved or unobservable processes which may be inferred with more or less confidence from the verifiable data.

One of the main undertakings of behavioral research is, of course, to make observable and verifiable many of those aspects of human action which have long been considered too subtle or too ill defined for observation.

Thus the "behavioral sciences" of today include characteristically the fields of psychology, psychiatry, sociology, and anthropology, more or less in their entirety. They make use of the methods and findings of such varied disciplines as mathematics, biology, linguistics, and the philosophy of sicience; and they include those areas and practitioners of the classical fields of political science, economics, and history, which are proving hospitable to the acceptance of behavioral research methods as contributors—however modest—to their work. This intellectual relationship has proved itself by now in practice, and the corresponding composition of the group of Fellows at the Center for Advanced Study in the Behavioral Sciences has turned out, on the whole, to be not arbitrary but functional.

The contributions of the behavioral sciences to the study of international relations are no longer a mere matter of programs for the future. A large volume of such contributions has been made in such topics as international communications, public opinion and propaganda, the effect of contacts among persons of different backgrounds of culture, language, race or nationality, the behavior of small groups of negotiators on committee members, the social and political effect of the growth of towns and industry, the development of intergroup prejudices and conflicts, the realistic and stereotyped perception of foreign countries and peoples, the formation of policies, the making of decisions, and many others.

The usefulness of the skills of the behavioral sciences has been recognized by governmental and private agencies in the field of international relations. During the Second World War, and ever since, there has been a demand for persons trained in behavioral research for many of the reporting and operating jobs which otherwise would have gone in most cases to traditionally trained students of political science and international law. Although the latter types of training continue to be of major importance, it seems safe to say that nowadays any student or practitioner of international relations, such as a foreign service officer, may expect to meet with an increasing number of problems to which behavioral research is applicable. At the same time, he is likely to find an increasing number of behavioral research results, such as public opinion polling data, summaries of interviews and surveys, content analyses of newspapers and radio broadcasts, statistical data, etc., coming to his desk, calling for his informed and critical evaluation. Increasingly any training which he may have received in the course of his university studies for this somewhat novel range of tasks is likely to be remembered by him with appreciation.

BEHAVIORAL RESEARCH AND THE SPECIAL ACADEMIC DISCIPLINES

Within this range of the behavioral sciences, each special discipline remains primarily a group of persons united with their fellow specialists by many ties. They share a common specialized training, are members of the same academic departments, attend the same professional meetings, read the same journals and much of the same literature. They cross departmental lines from time to time in order to collaborate with other specialists on some particular problem, but even though they may think of themselves as "interdisciplinary" and "prob-

lem oriented," most of them continue to find their major intellectual resources and scholarly contacts in their special fields. They continue to look to these special fields for informed criticism as a basis of their own professional reputation as well as of their own assessement of the professional reputation of others. The fact that most appointments and promotions at the senior academic levels in the leading universities continue to be made within the specialized departments, and on the basis of departmental criteria, contributes to this persistence of boundaries between the disciplines, but there is reason to think that these would continue to exist even if the formal academic arrangements were different.

It is desirable, therefore, that students of international relations should retain their professional identity. For most of them, this will be the field of political science, even though there should be opportunities for properly trained international lawyers, international economists, diplomatic historians, and perhaps in time some social psychologists or sociologists specializing in international relations.

The primary emphasis should thus be on the possible contribution of behavioral research methods to the training of students of the special discipline of political science, and within it in the field of international relations. The aim of the core of this training would be in general not to train graduate sociologists or psychologists, but graduate students of international politics, its cognate disciplines of international law and diplomatic history, enriched by the relevant contributions of behavioral research.

But how is this broader and more effective training to be accomplished for graduate students whose curriculum is already full?

COURSES IN BEHAVIORAL RESEARCH: AN OPPORTUNITY FOR CURRICULUM DEVELOPMENT

The best answers to this problem will not be immediately obvious. In the long run, it will require a rethinking of the graduate curriculum for training in international relations. A serious study, aimed at developing an integrated graduate curriculum which would unite effectively the traditional and the behavioral approaches to the subject, might well be desirable.

Certain things, however, could be attempted at once, even in the absence of such a major study, or pending its completion.

Within the framework of a specialized one-year program for a master's degree in International Relations, at least two one-term courses might be offered.

One of these could be an advanced survey course on "Behavioral Research in International Relations." This course would introduce students to the major sections of the large existing literature of behavioral research relevant to their subject, and it should acquaint them with the main research methods and results that are now available to the practitioner of international relations. The aim of such a one-term course should be not to give a one-sided training in the details of one or two behavioral research approaches, but to familiarize the student with the range of methods and data available, to the point where he can gain a coherent picture of their appropriate application to different problems, where he would know which methods to select as most promising in particular cases; and where he would know where to turn for competent help and more specialized information in regard to each behavioral approach selected.

The second one-term course should offer training in a substantive area of international relations, in which behavioral methods could be applied, jointly with historical and descriptive ones, to the study and analysis of concrete cases. A course on "Nationalism," or on "Nationalism and Imperialism" seems particularly appropriate for this purpose in view of the critical importance of its subject matter, and of the wealth of specific historical, descriptive, and behavioral data and literature available for it.

If more room could be found within a one-year curriculum, or, if the curriculum for a master's degree in International Relations were expanded to two years, a third one-term couse could be added, on "International Communication and Political

Opinion." This course would cover in greater detail between one-quarter and one-third of the topics which would otherwise have to be included in the Behavioral Methods course, it would thus leave more time in the latter for the remaining topics and for the study of combined research approaches. Alternatively, it would be possible to extend the Behavioral Methods course over two terms, grouping its subject matter so as to allow for an appropriate division. This would leave communication and opinion research included in this course, but would allow more time for each topic.

In the case of a two-year curriculum, a further strengthening of the behavioral component might be both desirable and practicable. Three further one-term courses might then be added to advantage. In order of importance and availability of research literature, these might be: 1. Applied Statistics for International Relations; 2. Psychology of International Relations; 3. Sociology of International Politics.

THE QUESTION OF USING OUTSIDE COURSES

As a compromise, in the absence of time or resources to develop the specialized courses just listed, some general graduate courses in Applied Statistics, Social Psychology, and Political Sociology, respectively, might be substituted for them. Such more general courses are available ordinarily at most major academic centers, and these might be taken for the time being by graduate students of international relations as part of their curriculum.

It would be desirable, however, if specialized courses could be developed in the application of each of these disciplines to international relations. Indeed, such an undertaking might form an appropriate part of any larger curriculum development study of the kind referred to earlier. In each of these disciplines there already exists a body of literature applying some of their methods and findings to political science and to international relations, which would furnish appropriate starting points. Until such specialized courses are developed, however, each student would have to select from the now available courses in the behavioral disciplines those most appropriate to his particular program.

POSSIBLE AUXILIARY FACILITIES FOR BEHAVIORAL RESEARCH

In the course of time, certain auxiliary facilities could be developed to increase the effectiveness of the behavioral research offerings within the total program. These facilities might include the appointment of a part-time Consultant in Statistics, as applied to social science and international relations, who could advise students on the most effective ways of dealing with quantitative problems in their work. Later on, another part-time consultant might be added for the field of social psychology, as applied to interview and group research design. Finally, the current bibliographical resources and library holdings might be strengthened in the behavioral sciences as they apply to international relations. Possible arrangements of this kind might have their place in a longer range attempt at curriculum development, and one or the other then might be approached within its framework on an experimental basis. Details on these points, however, as well as on the Behavioral Methods course discussed earlier, would go beyond the limits of this sketch.

A GRADUATE COURSE ON "BEHAVIORAL RESEARCH METHODS IN INTERNATIONAL RELATIONS"

Perhaps the best that can be done with very limited means would be to offer one broad survey course that would acquaint the students with the main approaches and techniques of behavioral research, as well as with the limitations, and that would indicate to the students where they could get more thorough information about any particular approach or method if they should want it. Its aim should be to give the students an impression of the range and multiplicity of behavioral research methods, and to their different potentialities and limitations. To acquaint them with only one or two, or to leave them under the impression that behavioral research is identical simply with opinion polling, or interviewing, or the use of statistics, would be to misinform them

seriously. The most important thing that students can learn from such a course would be that there are many practical, tested ways of supplementing our qualitative insights or intuitive judgments by the use of at least some verifiable data. Which method to choose, and which of the different possible approaches to combine, is then a matter for the judgment of the investigator in any particular research situation. The course could only offer them some starting points for such judgments, and it should not try to supplant them by an unnecessary narrowness in its selection of topics.

For these reasons, the proposed course would attempt to acquaint the students with approximately ten major topics of behavioral research, as it applies to international relations. Together with an introductory section and a concluding summary, these will add up to twelve topics for at least one week's work each, if the course is to be offered for a single term, or for an average of between two and three weeks each, if the course should be offered as a one year's course, extending over two terms running. A convenient dividing point would come, in this case, after the sixth topic.

A very tentative syllabus for such a course might read as follows:

1. *Introductory.* The Concept of "Understanding" and Its Several Meanings in the Social Sciences. Empathy, or what we imagine about other people's feelings. Part-whole understanding: How we interpret an event or a message from their context. Behavior: What we can observe of other people's actions. Insight: The identification of particular elements in a total situation. The use of concepts and conceptual schemes. Their "operational" meaning. The notions of cause, probability, and statistical significance. The use of qualitative and quantitative models. The combination of experience, evidence, and judgment.

2. *The Use of Existing Large-Scale Data.* Some major national and international organizations which collect and publish data relevant for international relations. How statistics are collected and aggregated. What is gained and lost by aggregation. Examples of data: Population, migrations, ethnic and language groups, income statistics, literacy data, ratios of rural to urban population, and of agricultural vs. industrial population; political participation; voting; government budgets, defense budgets vs. total government expenditure; etc. International flows of transactions and the data about them: the flow of travel, mail communications, and international trade. Data about mass communications: newspaper circulation and readership, radio, television and motion picture audiences.

Simple skills of data processing: the computation of ratios, trends, rates of change, stability and fluctuations, types of statistical distribution: normal, skewed, bimodal, etc. The simple projection of trends into the past for testing and into the near future for suggestions about conditions to be watched. Cyclical processes and the concept of auto-correlation. The mapping of flow patterns for indications of the strengthening or weakening of ties between two or more regions, or between several countries. The analysis of simple matrices.

Some graphic methods of presentation and their pitfalls. "How to lie with statistics"—and how to use statistics critically.

3. *The Analysis and Cross-classification of Data by Smaller Units.* The breakdown of large aggregations of data into smaller groups, such as by counties, precincts, income groups, educational levels, occupations, and the like. Their cross-classification with political data, such as votes for particular parties or policies. Example: Daniel Lerner's Cross-classification of French Parliamentary votes for or against the European defense community with data for the French Departments, showing that three specific geographic explanations for this outcome, each advanced by an experienced French political leader, such as Messrs. René Pleven, Georges Bidault, and Joseph Laniel, respectively, were all demonstrably incorrect.

Simple methods of regional or local mapping, statistical correlations, principles of punched card techniques for data sorting, levels of statistical significance of results.

4. *Principles of Sampling:* Using small-scale data to serve in the place of large-scale information. Inference from small samples to large-scale processes. Principles of the theory of sampling. Stratified and proportional samples. Sampling errors and methods for their estimation. Testing for reliability. Confidence levels.

5. *Community Studies for Particular Countries or Cultures.* The anthropologist's method of studying a "whole community." Its application to political problems. Some village studies in Western and non-Western countries. The application of community study techniques to towns and cities.

The study of values and beliefs as they operate in a community. Studies of child-rearing practices, prevalent personality patterns and "national character." Applications for attitudes toward authority and toward foreign groups. Patterns of local or national culture, culture contact and culture conflict, as they affect international relations. Personality patterns and political behavior.

Some problems of method: The selection of a sample community, the attempt to construct a total or near total "cultural inventory." The use of informants, field notes, etc. The study of cultures from a distance. The analysis of style in culture and in politics.

6. *The Study of Elites.* The relation of elite studies to the study of local or national communities. The concept of the "opinion leader" and of political "influence." Some problems in the measurement of influence. What data about an elite group would be most revealing? Income data, age composition, regional recruitment, occupational backgrounds, social status, career types, patterns of formative experiences. The concept of "political generations". Some recent elite studies: England, Germany, China, U.S.S.R.

7. *Public Opinion Research and Attitude Studies.* Attitudes and their measurement. Scales of attitudes and the conditions for their construction. Secondary aspects of attitudes: saliency, intensity, involvement, etc. Public opinion polls and their limitations. Some approaches to the measurement of meaning and of values. The relationship of attitudes to actions: Studies of voting intentions vs. actual votes; market research (purchasing preferences vs. actual purchases), audience research.

The application of statistical methods to opinion research. The search for representative samples and for smaller sample sizes. Principles and limits of factor analysis.

8. *Interview and Survey Methods.* Principles of interviewing. Focussed interviews vs. nondirective questioning. "Open ended" questions for saliency or intensity of attitudes. Depth interviews. Repeat interviews and panel studies. Special problems of cross-national interviews, and equivalent questions in different languages or cultures. "The art of asking questions."

Extensions of the interview approach: projective methods. Examples: The Thematic Apperception Test (TAT); the Rorschach test.

9. *Mass Communication Studies and Content Analysis.* The measurement of attention by countries or topics. Ratios of favorable, neutral, and unfavorable items concerning particular countries or issues in newspapers or radio broadcasts. Symbol counts, e.g., of national vs. international symbols. Tests for the political independence of publications. Qualitative content analysis.

Some techniques: item counts, space counts, trend plots, comparison of turning points. Some major recent studies.

10. *Small Group Behavior and Experiments.* Formal and informal groups. Task-oriented groups and task leaders. Are task leaders popular? The sociometric approach. What is a "sociometric star"? The problem of group integration and cohesion. Patterns of communication and decision-making in small groups.

11. *The Decision-making Process.* Decisions, values, and uncertainty. The initial information and its categorization. The formulation of the problem. The path of action. How decisions harden. Some examples of recent decision studies in the field of international relations.

12. *Problems of Application and of the Combination of Approaches, with References to Cases.* Summary and Review.

It will seem obvious that most of these headings could furnish topics for separate courses. Indeed, at most large university centers, graduate courses on several of these topics should be available. The purpose of this proposed course would not be to replace them. Rather, it could do at best three things: to help those students whose schedules, research interests or career plans do not provide for a more thorough pursuit of these more specialized topics; to help others to select those more detailed courses that promise to be most useful in their future work; and to help all students to get a sense of the need to combine and coordinate these different behavioral research approaches and methods with one another and with the more traditional but still indispensable approaches to intelligent judgment in international affairs.

(Manuscript received June 14, 1957.)

Error of opinion may be tolerated where reason is left free to combat it.

—THOMAS JEFFERSON, in his First Inaugural Address

COMMENT

Information and Control, a new Journal

An event of interest to behavioral scientists of interdisciplinary and system-theoretical inclinations is the appearance of *Information and Control*, a new journal edited by Leon Brillouin, Colin Cherry, and Peter Elias, assisted by an international Editorial Board.

The statement of editorial policy stresses the "horizontal" orientation of the journal, that is the application of general concepts developed in information theory and related fields to a variety of contents. Besides the "natural habitats" of information theory, such as theory of communication, theory of automata, and theory of automatic control systems, specifically mentioned are "Description and analysis of language and other natural information sources," "Communication and control systems which may include links within and between organisms," "Information aspects of physics and the theory of observation and measurement," and "Organization, processing, and retrieval of data."

The first issue amply reflects this program. In the opening editorial, L. Brillouin raises a number of highly provocative "metaphysical" questions (here "metaphysical" bears the same relation to "physical" as "metamathematical" to "mathematical") to which informational-theoretical concepts may well contribute clarification. Regrettably, the brevity of the editorial does not do full justice to the importance of these questions not only for philosophy but for science. However, the reader familiar with the issues, will appreciate the crux of the argument.

There are two papers dealing with language. G. A. Miller and Elizabeth A. Friedman discuss experimental results on the reconstruction of mutilated texts. It will be recalled that the facility of reconstruction is related to redundancy measures of the source. However the amount of mutilation tolerated (that is, the amount which still permits a perfect reconstruction) is usually much below the theoretical redundancy. The experiments discussed by Miller and Friedman show wide differences in reconstructability depending on the character of mutilation, e.g., substitution of characters, deletion, insertion, etc. For example, as much as 35% deletion of the most frequently occurring characters still allow an almost 100% reconstruction, but random substitution affecting 30% of the characters allow only about 30% reconstruction, etc. The results are discussed in relation to problems relating to accuracy and efficiency of telecommunication.

Benoit Mandelbrot undertakes a theoretical derivation from some plausible hypotheses of Berry's law, which relates the probability that a word occurring in oral discourse is stressed to the sum of the occurrence probabilities of all the words which do not exceed it in rank. He notes in his conclusion that the "fortunate insensitivity" of the derived results to the assumptions of the underlying model permit the construction of theory but that this circumstance has also an obverse aspect: the theory does not lead to very sharp or precise concepts which are to be used in the construction of models. This remark strikes at the very core of the challenge inherent in the task of putting statistical linguistics on a firm theoretical footing.

Application of an informational measure to statistics is discussed by E. H. Linfoot; suggestions for applying algebra of operators to macroscopic observation are offered by R. Vallee, and further contributions to the engineering-mathematical aspects to communication are made by C. E. Shannon, N. M. Blackman, I. Kay, and R. A. Silverman.

Students and workers in information theory will welcome *Information and Control* as an important channel of communication.

ANATOL RAPOPORT

Likert, R., and Hayes, S. P., Jr. (Eds.) *Some applications of behavioral research.* Paris: UNESCO, 1957.

THE most important feature of this book is its sponsorship. It is the second in a series on "Science and Society" being published by UNESCO. The first was *Economics and Action* by Mendes-France and Ardant. As a consequence it will appear in many libraries which previously did not acquire books on applied social science. It will get into many unexpected hands, particularly those of law, public administration, and humanities students and their predecessors who have now become politicians, civil servants, and executives. In many ways the editors recognize that the published volume may be expected to pioneer new routes in the transmission of ideas so that the contents already take its function into account.

Thus the book is particularly useful to the behavioral scientist in its effect upon the potential clients for which such research might be done. It is calculated to produce a more sophisticated user. Moderately extravagant claims are made for the returns from behavioral research (which includes here whatever aspects of psychology, sociology, economics, anthropology, and political science appear to be relevant to the organizational problem at hand), but the appropriate cautions are also to be found in each context for research that is presented. Thus it is pointed out here, and seldom in any other readily available source, how social researchers should accept projects from the managements of a social organization (usually a government department or a large firm), to whom they should report, and with whom they must find a basis for cooperation. There are warnings that behavioral research is likely to have a superficial impact unless fundamental research has been done previously, so that it is known what variables are most worth investigating, and abundant evidence is given of superficial results in the examples taken up in detail.

What might this new audience learn? There are reports on the sample survey approach to critical national problems such as investment in government bonds, and the responses of local people to nationally prescribed programs (e.g., conservation). They would discover that leaders both "initiate structure" in an organization and provide "consideration" for members of his group which serves to bolster morale, that close supervision by leaders more often than not is associated with low productivity, that training for leadership will depend upon the use of rate-paying and many ancillary techniques and may still not have more than a temporary impact, that research scientists appear to be most productive when their immediate superior is in the same field and their closest colleague is in a different field, and that creative scientists tend to be cosmopolitan and less interested in moving up the institutional hierarchy.

Perhaps the most closely read section will be Chapter Five, which analyzes the training of foreign nationals in the United States. In it they see a careful breakdown of the adjustment phases affecting students from overseas, what kinds of people seem to do well, and what kinds of policies may be undertaken which reduce the antipathies that are often engendered in those that pass beyond the "spectator" stage and reach that of the "sojourner" and still more advanced degrees of involvement. They would see, also, that the ability to learn English is not a good indicator of success, but that successful students with few exceptions have a good command of English.

There is more on reference group analysis and its usefulness in marketing, labor relations, politics, communications, and a chapter reviewing some of the uses of consumer surveys. The volume is concluded with a brief note on the methods by which seminars which bring together the researchers and the prospective users of research are brought together to discuss the implications of the latest results and lay out promising directions for new studies.

What may the potential overseas client get out of this report? That he will get a great deal is assured by use of graphs instead of tables, and the avoidance of techni-

cal jargon (although he will often be puzzled by use of contemporary American slang to characterize certain stereotypes). He will be puzzled by the lack of historical analysis in the problems being considered, because in societies where social change is slower paced the recent history is both more informative and easier to ascertain than a sample survey. This book is proof for them that the well-known thesis asserting that Americans lack a sense of history can be extended to its behavioral science.

He will also wonder how a Survey Research Center, modified somewhat according to the tastes of indigenous scholars, might be set up in his own cultural framework. Observers will be dubious that these techniques will be equally effective in other cultures, in other words, they are a science rather than an art cultivated by Americans. They may be reassured if they were provided evidence that American firms using techniques of leadership and work organization recommended from these studies can move into such disparate cultures as the Scottish, Welsh, provincial French, Sicilian and Mexican and still obtain remarkable industrial productivity within two or three years. He would be interested in the experiences of British social research group, the French, the German, and especially the Puerto Rican.

But the problems are so different, and the costs so disparate as against the returns in the poorer societies, that important extensions are still necessary. They have the task of bringing birth control to illiterate families, of controlling the growth of burgeoning cities, of converting agricultural workers into construction hands and village boys into mechanics, of assuring the flow of windfall profits into productive investments, and of developing a competent civil service. Obviously more books are needed for the same distribution channels because the contribution of the behavioral sciences to the really crucial problems has only begun to be indicated.

RICHARD L. MEIER

General Systems Theory from the Engineer's Point of View. Goode, H. H. and Machol, R. E. *System Engineering*. New York: McGraw Hill Book Co., 1957

WHEN this reviewer entered college, he was advised by the registrar, "Take physics; then you can do anything." In much the same spirit the reading of *System Engineering* by Goode and Machol can be recommended to the aspiring generalist. For in it he will find the most clearly defined approach to the behavior of organized entities viewed as systems, focused on the study of the complex interaction of several semi-autonomous parts. Whether an "organism" is a man-machine system, a man, a machine, or a group, there seem to be under- (or over-) lying principles governing the organized aspects of their behavior.

The degree to which these principles are made precise, operational, and relational (which usually means mathematical) rather than metaphorical distinguishes the generalist approach from the dilletantist or the interdisciplinary from the undisciplined.

The distinction is between talking and doing. In order to *do* general systems research, one must be able to handle the tools appropriate for it. From the engineer's point of view, these tools are honed by the mathematicians.

In *Systems Engineering* many of these new tools are described. As the authors state, "this is a practical book . . . ." They go on to indicate cogent objections to much of the output which purports to treat of general properties of systems: "It seems to us that abstraction has begun too soon. General theories are being propounded, with an easy reliance on analogy and generality. To the contrary, this book develops no general theory . . . ." The authors are being modest. They do treat behavior of a wide variety of systems in the light of quite general principles. They do not venture, however, to make any statement about "all systems." More specifically "the book presents experience, the parts and pieces, and the rela-

tionships among them; occasionally, in a small area, it attempts to tie several parts together with a general observation. Even so, based on present experience, the formulation of such general statements is difficult; when they are made, they are hemmed in with caution."

This book is written for engineers. It does not tell you what questions to ask or when to use the tools presented, but it does outline many methods of attack on many diverse problems. While no attempt is made to be exhaustive or even particularly rigorous, enough information is presented so that the reader may make an intelligent decision on whether a particular attack is applicable to problems with which he is concerned and to formulate his questions well enough within the language of the field, so that they can be communicated to the expert.

The topics are:

1. Probability theory (5 chapters).
2. Mathematical statistics.
3. Analogue and digital computers (7 chapters), including description of components, operation, and programming.
4. Queueing theory.
5. Game theory.
6. Linear programming, group dynamics, and cybernetics.
7. Information theory.
8. Servomechanism theory.
9. Human engineering.

The fields of application are telephone systems, air defense systems, automated factories, computers, etc., i.e., various types of man-machine "systems." Of crucial importance as the pivotal idea of the book is the authors' conception of system. A system to them is characterized by its *purpose*: "... all the parts of the system have some common purpose; in some sense, they all contribute to the production of a single set of optimum outputs from the given set of inputs ...."

The strong "teleological" flavor of this definition is, of course, entirely in the language in which it is given. As every biologist knows, teleological assumptions are not necessary to explain the optimizing characteristics of living systems. The engineer can, of course, use teleological concepts much more directly, because he always deals, at least in part, with man-made systems. It is not the "teleology," however, which is of essence (if I may be permitted to paraphrase the authors' meaning) but the optimization characteristics. Systems optimize in *some sense*.

Given this conception to start from, the task of the system *designer* is first to state the problem, to discover the "purpose" of the system. Second, he must define a "measure of effectiveness," that is, describe how he will know whether a particular design is doing a good job. Third, the designer must select, or devise, that embodiment of the purpose in machines and men that best (or sometimes merely adequately) fulfills the purpose, as determined by the measure of effectiveness. Each step is interlinked and (an important point!) a suggested solution may well change the preliminary statement of the problem.

Although a scientist, studying relations among events, does not necessarily have an engineering problem to solve, in the sense of designing a system, nevertheless, in seeking to "understand" a given system he will be better fitted to cope with *his* problems if he can bring to bear a thorough knowledge of the principles of systems design.

This book, therefore, should be both useful and instructive to all who deal with complex collections of parts or interactions of events which may be profitably examined from the system-theoretical point of view, as the authors of *System Engineering* understand it.

CAXTON FOSTER

ABSTRACTS OF CURRENT LITERATURE WITH INTERDISCIPLINARY IMPLICATIONS

Prepared by Members of the Staff of the Mental Health Research Institute

SIMON, HERBERT, A. **Amounts of fixation and discovery in maze learning behavior.** *Psychometrika*, 1957, 22, 261–268.

A simple maze is characterized by two parameters: the number of choice points, L, or length of the maze, and the number of choices that can be made at each point, denoted by n. A theoretical model is presented which allows the calculation of two performance parameters or "measures of learning" (namely the number of trials, T, and the total number of errors, E, to criterion) from the maze parameters. It is assumed that two independent processes are involved in maze learning: (*a*) a discovery process based on trial-and-error search for the correct response, (*b*) a fixation process to reinforce the correct response. The latter task of fixation is the same as the task of fixating the correct response in a situation of serial learning. In that case T is bound to be a monotonically increasing function of L (the length of the sequence), i.e., we have $T = f(L)$ with $df\ dL > 0$ or, in the simplest case, $T = bL$ where b is a positive constant. This same hypothesis is made for maze learning; it implies in particular that T is independent of n, which means that T does not depend on the amount of information obtained by making a decision at a particular choice point. The "universe of discourse" for assessing that amount of information is given by the total number of choices, n, that one could make at that point. The author discusses the plausibility of this "independence"; experimental results show that it holds strictly only for $n > 2$.

As for the discovery process, the following assumptions are made: (1) when the subject is at a particular choice point on a particular trial, he either knows or does not know the correct response; (2) if the former, he gives the correct response at once; (3) if the latter, he tries responses at random until he hits on the correct one. Using these hypotheses and a few empirical relations it is deduced that $E = a(n-1)\cdot L\cdot f(L)$, where a is a constant. If we put $f(L) = bL$ this becomes $E = \text{const.}\ (n-1)L^2$.

These theoretical predictions are compared with: (*a*) experiments by Brogden *et al.* on human learning in verbal mazes of various lengths and number of branch points; (*b*) a series of experiments with animals by Scott and Henninger using two-alternative mazes of various lengths. The agreement between theory and empirical data is found to be very good. (*ET*)

MARCH, JAMES G., **Measurement concepts in the theory of influence,** *J. Politics*, 1957, 19, 202–226.

The author undertakes to define the *relative influence* of two roles or of two behaviors. Roles are compared with respect to *a given behavior*, and behaviors are compared with respect to *a given role*.

The two-role situation can be analyzed by means of a matrix, whose rows are coordinated to the range of possible behaviors available to one role, and whose columns are similarly related to the other role. Each element of the matrix is an "outcome," a function of the respective behaviors chosen by the occupants of the two roles. "Outcomes" and "possible behaviors" are both defined in terms of whatever universe of discourse is relevant to the substantive analysis at hand. Aggregation or subdivision of "outcomes" or "behaviors" can radically affect the direction of relationship in the comparison.

In electing one specific behavior, the role-occupant thereby restricts the set of possible outcomes to those which lie in the corresponding row of the matrix (or column,

as the case may be). The size of this restricted set is the basis for defining the relative influence of that specific behavior, *with respect to the given role*. Of two behaviors, the one with the greater influence is the one which accomplishes the greater restriction upon the set of possible outcomes.

In the extreme case, any behavior which can narrow the possibilities down to a single outcome is maximally influential. Election of this behavior deprives the other role-occupant of any control over the outcome.

The definition is readily extended to a multi-role situation, in which outcomes are coordinated to the cells of a hypermatrix. The respective behaviors chosen by the several role-occupants determine the resultant outcome. Another extension enables the definition of the relative influence of two behaviors with respect to a *set* of roles, if they have the same relative order of influence with respect to each role in the set.

Success at narrowing the range of possible outcomes is also the basis for definition of relative influence among roles. If the occupant of one role, in electing a given behavior, thereby restricts the set of potential outcomes to a greater degree than would the occupant of another role, then the first role is more influential than the second *with respect to that behavior*. One role is more influential with respect to a *set* of behaviors if the same direction of relationship applies for all behaviors in the set.

It is possible for one role to be more influential than another with respect to one set of behaviors, and less influential with respect to a different set of behaviors. Or the relative influence of two roles may be contingent upon the behaviors adopted by occupants of still other roles. Similar statements can hold true for the relative influence of behaviors. Influence comparisons are indeterminate, in such cases, for the total universe of discourse. Nevertheless, the existence of *sets* in which one role (or behavior) dominates often permits a reduction in the complexity of the original data; one set supplants a number of cells.

The author's criterion of influence depends solely upon ability to narrow the set of possible outcomes; it ignores the value of the set to the role occupant. If this set is not as desirable as some other, less controllable one, then the role occupant has *negative* influence. The author has therefore deliberately excluded from his definition, because of the operational difficulties which it would entail, the principle of *intent*: whether the possessor of negative influence willfully conceals his true motives.

In addition to the verbal definitions mentioned here, the author provides precise definitions of relative influence in terms of measure theory. These give rise to a careful mathematical discussion of the various senses in which one might construe one set of alternatives as being "smaller than" another, together with the empirical implications of each.

[In his definition of the relative influence of roles, March refers to the differential effects of the same behavior when adopted by the respective roles. This, of course, raises the question of what constitutes the "same" behavior, as between different roles. The answer is straightforward enough in his hypothetical examples of political behavior, where President or Speaker can endorse or condemn a bill. But in other cases the correspondence may not be so clear, so that comparison of roles for relative influence would be inconclusive.] (*CHH*)

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ABOUT THE AUTHORS

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In the first volume of his biography of Freud, Ernest Jones wrote: "Strictly speaking, the language of physiology into which Freud attempted to transcribe the phenomena of psychology was in its turn rather the language of physics which had been grafted on to the data of physiology. Brücke and others of the Helmholtz school had made it their life's task, with varying success, to describe those data in the terminology of chemistry and physics and to apply the laws of those sciences to physiology, as far as they found it possible. There existed the hope, born with Herbart in the 1820's, and later strongly supported by Fechner in particular, that the same extension could be carried out in the field of psychology, and in his earlier years Freud also must certainly have cherished that hope.

"In the half century since then that hope seems to have remained fairly dormant, but of late is has revived under the stimulus of the new work being carried out in the field called cybernetics. It would be interesting to attempt a correlation between Freud's early attempt and the modern outlook."

In this spirit the present article re-evaluates psychoanalytic theory and investigates the implications for treatment in the light of the author's extensive clinical experience and current researches in therapy.

UNEXPLORED AREAS IN PSYCHOANALYTIC THEORY AND TREATMENT[1]

by Franz Alexander, M.D.

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I. THEORY

DIVERGENT views about the structure and functioning of the personality and the nature of the therapeutic process resulting in heated arguments and occasional schisms has been characteristic for our field since its inception. Although these differences often were expressed in polemics, resembling religious disputes of past centuries, it is reassuring that concerning the fundamental principles of psychodynamics they mainly consist in different emphases. Or they are of semantic nature, resulting from the difficulties in communicating with each other abstract and often vaguely defined concepts. It is not facetious to say that often we ourselves do not know what we mean, and therefore it is not surprising that we cannot precisely communicate our ideas with others. We use terms and concepts which lack the precision of the abstractions current in older disciplines. We are experiencing to a much larger degree the same difficulty as the physicist when he applies mathematical abstractions to actually observed phenomena. The geometrist operates with points which have no extension in space. The physicist applies these geometrical abstractions to matter which has extension. A discrepancy is thus unavoidably introduced. In our field abstractions are incomparably farther removed from the actual things to which we apply them. I shall illustrate this gap between current psychoanalytic abstractions and observational data and the unclarities of these abstractions themselves with examples concerning our most fundamental theoretical views.

The mental apparatus

We speak of ego, superego and id existing as distinct structures within a cohesive system. What we actually observe are only

[1] Delivered as the Sandor Rado Lecture at The Columbia University Psychoanalytic Clinic, November 7 and November 9, 1957.

certain functions which we attribute to certain organized structures distinct from others within a larger unit we call the mental apparatus. Since psychoanalytic theory attempts to be self-contained, it prefers not to include in its scheme physiological concepts. The mental apparatus is not considered as something existing in space—for example, in the brain—but is conceived as a functional system, an organization of certain psychological elements such as impulses, instincts, notions, ideas and values. In a vague fashion we of course assume that this psychological system is in some manner connected with the brain, but in building our psychological models we are not concerned further with the nature of this connection. And yet we speak of ego boundaries and use other spatial notions, such as excluding impulses from the system we call the "conscious" and relegating it to the "unconscious." Such analogical schemes are, of course, quite permissible aids to help the visualization of a system of abstractions. The danger lies in the fact that at the same time we deal with the mental apparatus as a system of psychological functions, we also use terms and concepts of structural nature, referring to spatial arrangements. The statement, "An impulse is unconscious," means, strictly speaking, that the impulse lacks a certain quality of conscious impulses, but shares with the latter certain other qualities. If we think strictly in functional terms, we may say that under certain conditions an impulse may lose or acquire the quality of consciousness. In structural terms we state that an impulse, as soon as it passes the conscious ego's boundary—a spatial reference—it acquires the quality of consciousness. The point is not whether one or the other description is correct, but only that it is necessary to recognize explicitly that the structural approach supposes a spatial theory, and therefore it combines strictly psychological (dynamic) and brain-physiological models. It is a hybrid, a psychophysiological model without being explicitly so. I mention this only in support of my previous remark that we do not always know precisely what we are talking about.

It appears to me that if we want to construct a pure *psychological model* of the mental apparatus, we shall have to restrict ourselves to strictly functional, or if you prefer, "dynamic," concepts and visualize the personality as a system of interacting impulses, inhibiting and motivating forces. How useful such a model would be is another question, but we must never forget that most psychological phenomena are perceived in time but not in space. This need not prohibit us from structural speculations, but we must frankly state that in doing so we trespass into brain anatomy and physiology. This is no sin, and I am convinced that the future model of the mental apparatus will be built on a frankly recognized combination of psychological and physiological concepts. Since the functions of the mind and the brain are connected or even synonymous, this conclusion is nothing but stating something which is self-evident. It is so self-evident that if it were not for our almost compulsive avoidance of mixing psychology with physiology while actually we are constantly mixing them, this statement would be superfluous. We in the field of psychology need not be ashamed to admit that the knowledge of personality functions requires both psychological and physiological observations, and psychological concepts are not sufficient to reconstruct a final model of the mind. If, for example, we speak of action we deal with a physiologically observed phenomenon. When we speak of motivation resulting in action we refer to something about which we have only psychological knowledge. Accordingly, even such a basic concept as action cannot be described in its totality in purely psychological or purely physiological terms. One part of action, namely its motivation—at present at least—can be described only psychologically; the motor behavioral results only in physiological terms. The biceps contracts responding to a nerve impulse from the motor cortex at the moment a person wants to slap someone's face. Without stating both the psychological impulse to slap and the slapping itself, the phenomenon is not fully described. I shall not, enlarge on the opposite error of the behaviorists, who compulsively avoid dealing with motivation, which they cannot directly observe.

This constant unavoidable mixing of physiological and psychological data is clearly seen if we review the psychoanalytic theory of ego functions. We differentiate between perception of external and internal stimuli as one of its basic functions. We find the anatomical and physiological substratum of these psychological functions in the structure of the sensory apparatus, which conducts and registers stimuli originating outside and inside of the organism. We speak of the integrative function of the ego and recognize its anatomical and physiological substratum in the cellular architectonics of the brain, in an hierarchical system of coordinating centers and communication lines. We speak of the executive functions of the ego and find its anatomical and physiological implementation in the motor apparatus. Is it not high time that we try more systematically to reconcile psychological with physiological data and abstractions, and give up the traditional attitude that while it would be nice if physiology were in accordance with psychological notions, yet we do not need to be seriously concerned with physiology in constructing a psychological model of the ego.[2]

Undoubtedly we know little of the physiological basis of such phenomena as repression, substitution, sublimation, turning impulses against the self, etc., and our first task is to describe them in psychological terms as we observe them or conclude to their existence. What has to be kept in mind, however, is that these are but phenomena within a complex communication system as visualized by the cyberneticists, and eventually they will be expressed in general principles which prevail in all communication systems. We are not rivals, but collaborators with the representatives of this new branch of science.

Instinct theory

The distance between actual observation and theory becomes even larger when we begin to hypothesize certain forces responsible for the constant dynamic activities going on within the structure we refer to as the mental apparatus. It is true we experience in ourselves forces at work in the form of motivations, and are aware of causal sequences between these motivating forces and certain biological functions. This is an excellent starting point, at least as good as the Cartesian edict "cogito ergo sum." We feel hunger and as a result we walk toward the icebox. We can justly say that the sensation of hunger is in causal relation to our motor behavior, the steps we took toward the icebox. We feel an impulse we call curiosity and open the newspaper to learn about the satellite. The difficulty arises when we attempt to construct abstractions concerning these different impulses. As in every science, we begin with classification. We lump together activities motivated by hunger, self-defense, and our other goal-directed and structured activities, by which we try to satisfy our survival needs, such as going to school, then to college, then to a professional school. All such activities we attribute to a self-preservative (or ego-) instinct. We differentiate them from sexual impulses, and from all those idle, non-utilitarian curiosities and play activities, which are pursued, as it appears to us, for their own sake for the pleasure we derive from them. We have thus constructed our first two categories: the self-preservative and the erotic impulses. We soon discover the difficulties of making clear-cut distinctions. Nature is not willing to be fitted into our neat categories. Being curious about the content of an icebox is obviously different from the curiosity we have about the structure of the atom. The one is highly practical and is aimed to eliminate a basic need—hunger—which is obviously a self-preservative goal. On the other hand, even the most rabid pragmatist, the representative philosopher of our utility-oriented era, would concede that the curiosity about the structure of the atom originally was not directed primarily toward any practical aims. It was a l'art pour l'art kind of curiosity, even though the satisfaction of this curiosity later proved to have undreamed of practical applications. It is more similar to a child's prying open a clock to see how it works inside. We arrive then at two kinds of curiosity, one subordi-

[2] In collaboration with Ralph Gerard, we had made the beginnings of such a reconstruction which we plan to complete in the future.

nated to survival, the other a curiosity for its own sake, which has some erotic or at least playful connotation. Our classification system is now seriously threatened. We have to place the impulse "curiosity" into both of our pigeonholes. Sometimes it appears as self-preservative, sometimes as erotic. We may draw some consolation from theoretical physics, where light in some relations appears as corpuscular, in others as undulatory. But we meet the same difficulty even with simpler motive forces. We clearly recognize that to crave for a piece of bread when we are hungry, and to yearn for some culinary concoction are two different things.

Our dilemma increases when we proceed in our abstractions and try to conclude to some kind of a substratum behind these two kinds of impulses. We call them the instinct of self-preservation and libido. How to apply this classification, however, when the same impulse to ingest food and liquid once appears as self-preservative and the next moment as libidinous? Have they two kinds of material substrata, for example, two kinds of fluids or hormones, or do we deal here with two kinds of nerve impulses only in a quantitative, not qualitative sense? Lust for culinary pleasure and hunger mostly appear together in every day life: we eat both to satisfy hunger and enjoy the taste of food. One is rarely only hungry or only a glutton. As a solution, a kind of fusion of two instincts was assumed; Freud speaks of the erotization of an impulse: self-preservative and sexual impulses may mix and our job becomes to account for the proportion in which each of the two instincts participate in the fusion (11). The impossibility of making such a quantitative analysis sufficiently precise shows that this is little more than a "façon de parler."

A new difficulty arose when the two instincts were conceived as polar opposites. Freud always considered ego instincts and libido more or less as antagonists, and in his later instinct theory this antagonism became essential (14). He distinguished between life and death instincts opposing each other, and spoke of their fusion in terms of neutralization, the upbuilding life instinct neutralizing the destructive effect of the death instinct. Hunger now implicitly became a manifestation of Eros as a biological urge to maintain life. It is true that hunger and the erotically tinged culinary satisfactions coexist peacefully and do not disturb or counteract each other. Yet a valuable distinction between hunger and the erotic pleasures of the sense of taste was lost when hunger became an expression of Eros.

Since both kinds of impulses, Eros and Thanatos, appear in the organism, they must have some kind of a biological substratum, and if so do these two instincts have basically different biological substrata or only differ in their direction, two forces with opposing vectors? Freud's reference to anabolism and catabolism as the basic biological substrata of the two instincts implies a vector concept. Physiologists speaking of anabolism and catabolism do not assume two fundamentally different processes; they think of two phases of a physicochemical process called metabolism, which consists of building up more complex organic compounds and their simultaneous decomposition, which is connected with transformation of chemical energy into mechanical energy and heat.

It would be indeed attractive if one were able to reduce the great variety of purposeful and utilitarian activities preserving life, together with all playful erotic and frankly sexual phenomena on the one hand, and destructive hostile impulses on the other hand, to anabolic and catabolic processes; to understand hate and love, life and death as the ultimate manifestations of these biological antagonists. This dualistic theory has a grandiose metaphysical sweep in accounting for the two most fundamental biological and psychological phenomena: life and death and love and hate, as the results of a force operating in the direction of growth and organization, and another one aiming at destruction and disorganization. This theory, however, just because of its lofty abstraction can hardly satisfy those more concrete intellectual needs which led to the original differentiation between self-preservative and erotic phenomena. It hardly can account, for example, for the two forms of curiosity, one motivated by survival needs, one by a de-

tached urge to find out something only for the sake of understanding it without any useful purpose. Neither of them can be attributed to the death instinct. Similarly, hunger and culinary desire, both satisfied by ingestion of food, hardly can be traced back, one to a death instinct, the other to Eros. This impasse is seen most strikingly when we examine the phenomenology of the aggressive-destructive impulses.

Historically, the phenomenon of sadism was the stumbling block in Freud's original view, differentiating between erotic and self-preservative forces. Aggression can be both self-preservative and, in the form of sadism, erotic. As will be seen, Freud's solution of this difficulty led to new problems. The fact that pregenital manifestations of sex are not race preservative induced Freud to make one of his most lasting generalizations. Propagation, the mature genital act and the corresponding psychological attitude, the love felt for the sexual partner, is only one, the most advanced form of sexuality. It is the result of a development from immature pregenital, auto-erotic activities to mature race-preservative manifestations of sexuality (13). Although not explicitly stated by Freud, the pregenital manifestations of sexuality are the psychological expressions of organismic growth. They consist of playful and pleasureful practices of the different functions of the body, oral and anal practices, exercise of the muscle system and of the sensory organs. They do not directly serve survival; they, however, prepare the organism for independent existence. During these pleasurable playful practices of the different organic functions, the child not only derives "functional pleasure" but at the same time learns how to use these functions. A common example is muscle eroticism. In the mature organism these functions largely, though not entirely, lost their erotic nature and the sexual sensation became gradually transferred and restricted to the genital organs.

In his earlier instinct theory, not considering any longer propagation as an indispensable part of sex, Freud was able to enlarge the category of sexual phenomena and include into it pleasure-yielding practices of the body which have no race preservative function, yet experientially have a distinct similarity with genital pleasure. This way, phenomenologically at least, he arrived at two distinct categories—libido which includes all those impulses which are connected with a pleasurable sexual kind of gratification, and those useful activities by which we preserve our existence: the ego instincts. The dilemma, which this first instinct theory never could resolve was in which category should aggressiveness be placed. In the form of sadism it obviously has a sexual connotation. Sadistic acts do not serve survival needs—they are practiced as a source of sexual gratification. Aggression, however, may be devoid of any sexual connotation, as in a man struggling to overcome an obstacle which prevents him from obtaining food or wanting to eliminate a foe who threatens his survival. Yet, aggressiveness which was the prototype of an ego-instinct, under certain conditions which were never clearly defined, may appear as sadism, a frankly sexual phenomenon. A similar contradiction appeared with the introduction of the concept of narcissism. Self-love, as the classical reference to Narcissus clearly indicated, is placed into the category of libidinous phenomena. At the same time it is clearly an ego instinct. Obviously, the transition of an impulse from the self-preservative to the sexual category is a rather common phenomenon, and as long as the theory does not account for the factors which are responsible for the transformation of rational self-interest into erotic self-love—narcissism—or vice versa, nor for the factors responsible for the way rational self-preservative aggression assumes a libidinous appearance in the form of sadism, it is not a satisfactory theory. Freud's newer life and death instinct theory tried to solve this dilemma by substituting for the original pair—ego instinct versus libido—another antagonistic pair of impulses: Eros and Thanatos. Eros includes the self-preservative functions, growth and those functions which phenomenologically appear as erotic.

Now, narcissism, object love, all manifestations of pregenital and genital sexuality, creative activities on a sublimated level are

all considered erotic; aggression, whether directed against others or the self, is the manifestation of the death instinct. To account for the fact that self-destructiveness in masochism may be the source of sexual gratification, or destructiveness directed against others may frankly become sexual, Freud retained the auxiliary hypothesis of instinct fusion which he used already in his earlier theory. Here again the theory fails to tell about the factors responsible for such fusion. If ultimately the destructive and love instincts are manifestations of two polarly opposite vector quantities, such as cataboism and anabolism, it is difficult to conceive of any other kind of fusion but an arithmetical one, addition or subtraction. Destructiveness becomes accordingly sadistic by an admixture of Eros which minimizes the effect of Thanatos. All the self-preservative functions now become by definition, erotic, since they are serving life and growth. If they are all erotic, what then differentiates hunger from thumbsucking, practical from creative curiosity, muscle exertion for survival from the erotic enjoyment of muscle action? What is fused here with what? Obviously, not Eros with Thanatos, since neither the frankly erotic nor the phenomenologically non-erotic manifestations of these functions can be considered as expressions of the death instinct.

It is needless to belabor the point further that from these global abstractions there is no return to the concrete facts of observation.

The Freudian dual-instinct theory has, however, a nucleus which, if differently stated, resembles recent formulations of communication theory. Instead of assuming two kinds of instincts, one towards life an one towards death, it is more promising to speak of a trend towards organization, which counteracts the entropy principle, the most universal law of all natural process: the trend from more organized towards less organized states. As was pointed out by Wiener (30) and Schroedinger (27), some kind of device similar to the demon which Maxwell thought up to illustrate the entropy principle actually operates in living organisms. Maxwell's imaginary demon is located in a container filled with gas, watches the molecules and separates them according to their velocity by opening a trap door for the high speed molecules and closing it against lower speed molecules. This way he could increase the temperature on one part of the container and create order in the randomly distributed molecules with different velocities. This results in increasing free energy (negative entropy) of the gas. The internal organizing principle is obviously information about molecular velocities which enables Maxwell's demon to bring order into a chaotic conglomeration of elementary particles. In living organisms through all kinds of feed-back mechanisms such informations are actually received by a central agency, which makes it possible for the organisms not only to maintain stable conditions but even to grow and progress towards higher and higher states of organization. A living organism is one which actually possesses a Maxwell demon operating in it.

All this at first seems contradictory to the general entropy principle. The obvious answer to this paradoxon is that the law of entropy is only valid for closed systems. The demon depends on external sources of energy and his activities themselves involve at least as much increase of entropy as they save. In open systems—and actually all existing systems are open except the universe as a whole—temporary local deviations from the law of entropy are quite possible. Only the whole of the universe is running down towards thermodynamical equilibrium, in which there is no longer any free energy and no change occurs anymore, a state of a completely random distribution of all constituent parts within the universe. Communication theory now defines the instrument by which this continuous progress towards increasing entropy, that is to say, towards disorganization, can be at least locally and temporarily reversed in certain systems. This instrument is information. Shannon (28) has introduced a quantitative yardstick of information, one bit of information being equal to what is necessary to decide between two a priori equi-probable alternatives. This is indeed an exciting discovery because it connects entropy with communication theory; it

connects two seemingly completely separate fields; thermodynamics and communication theory. This connection is, however, not surprising. Weaver explains it by reminding us that information in communication theory is measured by the amount of freedom of choice we have in sending messages. In a highly organized state the degree of randomness of choice is low and also entropy is low. By means of information, that is to say by self-regulating feedback mechanisms, we can decrease the freedom of choice; in other words, we introduce order into a disorganized state just as Maxwell's demon can do. Information, operating within a system, accordingly acts against the law of entropy, is capable of destroying a certain amount of it, even though it in itself depends on external sources of energy. Along these lines of reasoning, communication theory might be able, if not to explain but at least to measure quantitatively, the degree of organization. Since organization achieved by means of information is an indispensable property of life, the degree of organization appears to be the proper yardstick for the quantitative study of the life process. The significance of this yardstick is that it has been found useful both in communication theory and also physics. As we shall see, it may have in the future an equally meaningful application in the study of instinctual discharge phenomena.

Instinct theory, communication theory and the law of entropy

No matter how stimulating this formulation of the most basic property of the phenomenon of "life" may be at the first blush, in itself it does not seem to help us to describe and understand better the relationship of sexual to non-sexual instincts. No such contribution came as yet from this source. The replacement of Eros and Thanatos with the more operational concepts of thermodynamics and comm nication theory is gratifying indeed, yet the gap between communication theory and instinct theory is still unbridged. In the following no attempt is made to close this gap in an operational sense; only a perspective of future possibilities is suggested.

I attempted in past writings (3) to describe the relation of sexual to non-sexual phenomena. Thomas French independently came to quite similar conclusions (10). I took as a starting point the fact that an extreme variety of functions, such as ingestion of food and all oral activities, elimination of waste material, curiosity, love, aggressive behavior, self-assertion, the wish to be noticed, can assume a sexual connotation, but also may appear as non-sexual under other circumstances. In other words, almost every emotion and voluntary function may become the content of sexual excitation. This is most clearly seen in the field of sexual perversions.

As stated before, aggressive hostility may appear as a completely non-sexual striving, but also may assume a distinct sexual character in the form of sadism. The same is true about curiosity which in sexual form appears in the voyeur. The wish to attract attention, to impress, in its sexual form appears as exhibitionism. In the history of passive homosexuals we often find an early frustration in the form of the longing for a strong father figure. Here the passive longing finds a sexual expression.

At this point I should like to call attention to another observation which throws further light upon the relationship of the non-sexual and the sexual expressions of the same impulse (3). A quantitative complementary relationship exists between these two kinds of manifestations. Persons who have manifest perversions frequently show a conspicuous lack of the non-sexual expressions of the same trend which appears openly in their sexual behavior. Sexual exhibitionists are often highly inhibited in their ability to make an impression on the non-sexual level. They conspicuously neglect their external appearance and are inhibited in such expressive innervations as gesticulation and modulation of speech. Although I have no statistical data available, I found a number of stammerers among them. Others did not have a definable speech defect but had marked difficulty in verbal expression. It appears that the crude exhibition of their genitals is the only way in

which they can gratify their need to be noticed.

Another example for this complementary relationship between sexual and non-sexual manifestation of the same drive is offered in sadism. It is commonly known that sexual sadists are often weaklings, incapable of forcefully asserting themselves in their relationship to others. Their aggressiveness is pent up because its normal expression in interpersonal relations is inhibited; it is discharged—short circuited—in their sexual behavior.

A similar observation pertains to pedophiliacs who are extremely inhibited toward women of their own age because of excessive inferiority feelings. Their only outlet, in many cases, is sadistically distorted sexual approach to minors.

In the analysis of voyeurs, I have been impressed by the conspicuous absence of any non-sexual curiosity. A counterpart of this observation was provided by a scientist who devoted himself to basic research. His passion to uncover and disclose the secrets of nature took the place of sexual curiosity which had become inhibited at an early age; only in late adolescence did he discover the anatomical difference between men and women.

In the case of masochism, such a correlation was explicitly stated by Freud: (19) "An individual may, it is true, preserve the whole or a certain amount of his morality alongside his masochism, but, on the other hand, a good part of his conscience may be swallowed up by his masochism." Giving heed to this statement, I was impressed to find an extreme, sometimes blatant, selfishness sexual masochists display in their non-sexual human relationships. Their need for punishment finds sexualized expression in their masochism and this frees them from the inhibitory effect of their conscience.

Practically every emotional trend, love, curiosity, the need to call attention to one's self, aggressiveness and the need for suffering can be expressed either non-sexually or sexually. I have suggested in other writings that when an impulse is discharged in an isolated fashion without being subordinated to a more complex goal structure, it assumes a sexual connotation and that such isolated emotional discharges occur in states of excess excitation, when there is a surplus of motivational cathexis beyond that which is necessary for coordinated utilitarian, self-preservative functions (1). An excess of oral craving, beyond what is physiologically conditioned by hunger, is precisely what we call oral eroticism. Excessive accumulated aggressive impulse which is not needed for utilitarian aims—aggressiveness which is not necessary for survival, may be discharged in the form of sadism.

Such an excess excitation occurs mainly under two conditions: (1) when there is an amount of excitation in excess of what is needed by the organism in its survival activities, and (2) when excitations which could not be absorbed and included in the structure of the ego, because of a neurotic interruption of normal maturation, find isolated discharge either in the form of perversions or in neurotic symptoms.

The same surplus theory of sexuality applies well also to the genital manifestations of sexuality. It is in accordance with current biological concepts to consider reproduction as growth beyond the limits set by individual growth potential. This theory is in accordance with the biological view that growth and sexuality are closely related phenomena. Gerard, for example, defines the relation of growth to reproduction: "Clearly the defining line between growth and reproduction . . . is uncertain and broken. Reproduction of cells constitutes growth of the individual; reproduction of individuals . . . growth of the community. Indeed, reproduction might be looked upon as a form of discontinuity or a critical point dividing an otherwise continuous growth process into a series of separate quanta. . . . The simpler forms of reproduction, the division and separation of cells commonly but not always with mitosis, are merely such a punctuated protoplasmic increase" (22). In a sense, reproduction is the manifestation of an excess of "growth energy," of which no further use can be made in individual development after the organism reached the limit of its maturational process.

The fact that germ plasm remains iso-

lated and does not become an integrated part of the total organism also accords well with our psychological formulations. According to these, the sexual nature of an emotional discharge depends upon a special condition: whether the discharge takes place as an isolated gratification of a tendency which serves as a goal in itself, or whether it becomes a component part of a goal structure which serves the interest of the total organism. Although in genital sexuality the partial drives become integrated and subordinated to the goal of propagation, genital sexuality remains a goal in itself and is not necessarily subordinated to other goals. Genital impulses may also become desexualized and, like any other emotional trend, integrated into the total personality; they then find expression in non-sexual love, creativity or some other sublimated form. This desexualization, however, does not mean losing an admixture of another instinct—libido—but rather a different form of discharge of the same impulse.

Without trying to speculate about a special substratum such as two instincts of different quality, we ask the question, under what circumstances does an emotional content assume the quality of sexual excitement. The answer which our observational data give us is of great simplicity. Every gratification of an impulse has an erotic character if it is performed for its own sake and is not subservient to the needs of the organism as a whole. This can be considered as a surplus of excitation.

This view can be subjected to empirical scrutiny and does not need to remain the result of merely deductive speculation. If the statement is correct, we do not need to postulate two kinds of instincts of different quality. We consider sexuality in all its multiple manifestations as a special form of discharge of any impulse. The same impulse of the same quality can be discharged sexually or otherwise. It loses the quality of sexuality as soon as it becomes a constituent part of a complex structure—which French appropriately called a goal structure (10).

This concept of sexuality, though it was previously not stated quite as explicitly and comprehensively, is not quite original. Its sources can be found in some of Freud's early views on the nature of sexual phenomena, and more specifically in some of Ferenczi's writings.

Freud in his earlier writings several times raised the question whether sexuality was not merely characterized by a quantitative factor (13). In his "Three Contributions to the Sexual Theory", he wrote ". . . sexual excitation arises as an accessory to a long series of internal processes when the intensity of these processes has exceeded certain quantitative limits." Later in his "Instincts and Their Vicissitudes" he asked: "Are we to suppose that the different instincts which operate upon the mind but of which the origin is somatic are also distinguished by different qualities and act in the mental life in a manner qualitatively different? This supposition does not seem to be justified; we are much more likely to find the simpler assumption sufficient—namely, that the instincts are all qualitatively alike and owe the effect they produce only to the quantities of excitation accompanying them, or perhaps further to certain functions of this quantity. The difference in the mental effects produced by different instincts may be traced to the differences in their sources" (20). Here Freud, however, did not recognize that in addition to intensity the mode of discharge is essential. He refers, however, in addition to intensity, to rhythm. A more explicit statement comes from Ferenczi (8). . . . "whenever an organ fails to indulge its pleasure tendencies directly but renounces these in favor of the organism as a whole, substances may be secreted from this organ or qualitative innervations be shifted to other organs and eventually to the genitals, it being the task of the latter to equalize in the gratificatory act the free-floating pleasure tensions of all the organs." Sexuality consists in. . . . "all those accumulated amounts of unpleasure which, side-tracked during the utility functioning of the organs, were left undealt with, undisposed of. In ejaculation all those (autotomic) tendencies are summated, the carrying out of which was neglected by utility functioning. Every organ has its own individuality, its physiology of pleasure in contrast to its physiology

of function. The summation of those excitations which have no utility constitutes genital sexuality, which releases accumulated, unused energy." Ferenczi's formulation, if one disregards the non-essential statement about hypothetical fluids, is identical with the view here proposed.

This view concerning the relation of sexual to non-sexual phenomena has still one other commendation. The introduction of this theory closely resembles certain developments in the theory of heat and electricity. The earlier theories in both of these fields were substance theories, both heat and electricity were considered a special kind of fluid. In the course of time both of these "substance" theories gave place to purely dynamic concepts, the kinetic theory of heat and the field theory of electricity. In these newer theories heat became the manifestation of molecular movements; temperature depended upon the velocity of the movements, and similarly the electric field with its purely quantitative (geometric) characteristics replaced the fluid concept. Also the concept here presented no longer requires the assumption of two underlying qualitatively distinct instincts. We operate only with two modes of discharge of any excitation which can be characterized by two variables: intensity and, what is more important, a configurational variable. The latter depends on whether the discharge is a part of a structure of impulses subordinated to the survival interests of the total organism, or remains isolated and is discharged as an individual impulse.

The anatomy and physiology of the genito-urinary apparatus and the biology of propagation are strongly in favor of this interpretation of sexual phenomena.

Phylogenetically in the cloacal state there was one common organ for the elimination of waste products and germ cells; later it became divided into a urinary and genital portions, but its basic function was retained: all products which were not utilized for self-preservation, waste products and germ cells were drained by it. In addition, the genital apparatus discharges not only germ cells but also excitations which are not subordinated for the aim of survival. In the early phases of development, sexual excitation is pregenital and often extragenital and can be relieved locally, in the mouth, anus, etc. Gradually more and more of the erotic excitations are discharged through the genito-urinary tract. Every gratification has intrinsically an erotic character, which is satisfaction of a tendency for its own sake and not subservient to the needs of the total organism. It serves only to relieve surplus tension. Every surplus excitation which the organism cannot use or does not need for its own preservation is erotic and most of it is drained through the genito-urinary tract.

The view here presented accounts for the fact that every mental content can have a sexual and non-sexual expression and states precisely the conditions under which an impulse is discharged sexually or non-sexually. It makes superfluous to assume two specific instincts underlying the great variety of impulses.

It must be by now obvious that this view uses a somewhat similar concept as the one which has been found so productive in communication theory and physics: the degree of organization. It attributes the erotic nature of a discharge to its degree of freedom versus its being a part of a system of discharges. This can be also seen in society which gradually is losing its playful hedonistic qualities as it becomes more and more organized and thus restricts the freedom of the activities of its members. As Huizinga in his *Homo Ludens* so aptly stated, our present industrial civilization compared with earlier centuries is becoming more serious and less playful. As he puts it, in the 19th Century all Western humanity donned the "boiler suit". Play requires utmost freedom of choice, which is lost when the activities of man become closely knit into a social fabric (23). Cats and dolphins are great individualists and at the same time the most playful animals—ants the most collective and most organized and the least playful.

The maturation process of the growing child displays the same principle. As I pointed out before, the infant at first practices most of his biological functions playfully for the mere pleasure he derives from

them (1). The hands grab for the sake of the pleasure derived from it, the eyes focus on objects, not to espy useful objects in the environment, but for the sake of the sheer fascination derived from the act of seeing. The same is true for locomotion. The young colt, as the child, is romping around aimlessly for no practical reason but as a pleasurable expenditure of overabundant energy. In all these playful activities there is a great freedom of choice; they are not directed toward specific utilitarian goals which limits choice because the act is subordinated to and prescribed by the specific goal. Gradually all these separate non-utilitarian functions become integrated into rational planful activities and at the same time they lose their erotic quality. Our previous formulation about the nature of erotic discharge can be quantitatively expressed by saying that the *erotic value of an action is inversely related to the degree to which it loses the freedom of choice and becomes coordinated* and subordinated to other functions and becomes a part of an organized system, of a goal structure. If this is true and if the essence of life consists in the entropy reducing capacity of the organism, we cannot refrain from saying that the process toward increased organization or less freedom of choice takes place at the cost of erotic gratification of the individual members of a system, be these organic functions of the body or members of a social organization. In the insect states, for example, in termite society, organization progressed so far that the majority of the members became asexual and what erotic expression remains for them consists in an occasional communal ritualistic performance consisting in to and fro rhythmic movements collectively performed. Does the increasing trend toward spectator sports in our society express the same trend? From this perspective it appears that decreasing entropy is a rather costly process, costly for the constituent individual members or a system. It is not costly in the terms of energy consumption since communication processes operate with small quantities of energy, but costly in another sense, because the individual members of a system must sacrifice much of their freedom of choice and with it much of erotic pleasure. Sexual functions in insect societies are restricted to the small number of sexual types, in the organism to the genital tract and the germ cells. The latter remain isolated from the rest of the organism, are not influenced by the events within the organism to a great degree and are not interacting parts like other cells of the total system. With the individuals—cells in the organism, persons in society—sacrificing their freedom, the whole system's free energy, or to use a current expression, negative entropy, increases. Of course this increased negative entropy eventually will have to be lived down in the course of life. Moreover, according to current views, the organizational process itself takes place at the cost of negative entropy.

I shall stop here, no matter how tempting it may appear to work out a quantitative formula in which erotic discharge, or more precisely, erotic potential is measured by entropy, the same concept which was found valid for information. The difficulty to arrive at such a formula is that in the field of information theory we can precisely state the degree of choice, which we cannot do precisely at present in the case of impulses.

At our present state of knowledge operationally the loss of freedom of choice in impulse discharge cannot be measured, and therefore this type of reasoning may be considered as idle speculation. A quantitative formula increasing loss of freedom of choice in behavior where not simple messages but meanings are involved would account for the basic rhythm of the life process which consists in the parts continuously losing freedom during maturation in favor of the total system's freedom. This latter, appears, for example, on the social level as the emergence of the sovereign state, monarch or dictator, a particularly characteristic feature of the highly organized and centralized nations.[3]

The degree of freedom of choice in instinctual discharge we may call the "erotic potential." In view of the lack of an operational

[3] It is essential to distinguish between freedom of the constituent parts and the freedom of the system as a whole.

definition of this concept it would be premature to relate this to free energy (negative entropy). It is, however, not premature to consider the essence of life in the faculty of living organisms to reduce temporarily entropy by organization achieved through intercommunication between parts which reduces the freedon of their action. By this device organisms are able at least temporarily to postpone the unavoidable eventual decay of internal order, leading to a random distribution of their constituent parts.

I do not feel competent to answer the objection that all this is nothing but analogizing. Loss of freedom of choice of the constituent parts has a concrete quantitative meaning and can be measured in a variety of systems, such as a conglomeration of molecules in a gas or in an electronic communication system. At present it has no operational application to instinctual organization in a biological organism or to society. If a common quantitative operational definition of the degree of randomness actually could be forged, which would be also applicable to organismic behavior, this would unify all the different fields which are dealing with complex organized systems.[4]

It would be a naive conclusion, however, to state that since organization is so essential to life, the more organization the better. It is a different matter when certain privileged individuals may retain freedom of expression and thought or that of propagation and form an intellectual elite of scientists and artists or breeding types—a kind of brain cells and germ cells of society. The rest of the population is restricted to prescribed functions as, for example, muscle cells, which can only do one thing, contract and extend, or liver cells, which can only secrete more or less of a chemical substance.

At this point a highly subjective remark may be at place. From the point of view of the individual person, he is fully entitled to resist the trend toward increasing negative entropy achieved by organization, as far as he can afford it in view of the fierce competition between societies. Let us not forget that highly complex structures are apt to explode and are subject to a continual natural decay. At this point, the vistas of science and values diverge. Science cannot decide for you whether it is preferable to be the single electron of a hydrogen atom or one of the many that circle the nucleus of uranium; whether it is a more preferable existence to be one of the many identical liver cells enjoying complete routine and security or to live the exciting life in being a part of the brain tissue constantly facing change and being called upon to make fateful decisions, or a germ cell which through the completely random process of mutation can playfully produce an immense variety of fantastic new combinations of genes—a truly creative act of nature.

It appears that the need for increasing organization is superimposed from the outside upon all systems. Nations, we know from experience, organize and restrict freedom in war. It is true that Maxwell's demon rests within the system and performs his nefarious activities of ordering around the freedom-loving molecules on his own account, but as has been pointed out, he has to rely on external sources of energy for his own functioning. Moreover, the demon only directs and thus organizes the movements of the corpuscles, the individual momenta of each particle which they do not receive from the organizer. These dynamic quanta form the raw material of which the demon can build up his system. Organization is only one part of the creative act, there must be something to be organized and this is the unalienable property of each individual part. In any case, organization on all levels of systems, atoms, cells and societies, has to contend constantly with the basically never fully renounced trend of these elementary parts to regain their lost freedom for random activity. If this were not so, the universal trend in nature toward increase of entropy would not exist.

If the universe as a whole is considered a

[4] In a highly contestable writing on logical and general behavior system theory, R. C. Buck criticizes efforts of Gerard and Miller to find common principles prevailing in systems of different levels. This criticism would also apply to the here proposed general principle. I do not intend in this paper to discuss the heuristic value of this type of inferences.

closed system, this means eventual thermodynamic equilibrium or death. For pessimists, who assert this, however, there is one consolation, namely, that for all existing organized systems after their disorganization there is always a new beginning. External sources bring about a new impetus for the particles to form a new order only to be lived down again. The eternal rhythm between upbuilding and destruction goes on, if not forever, long enough for the inhabitants of this planet not to be concerned. And finally, is the universe a closed system?

Be it as it may, so far as the human race is concerned it appears more likely that it will become extinct not as a result of the law of entropy, but as the result of becoming progressively organized into two opposing camps bent to destroy each other. It is difficult to decide which one is the evil spirit, the individualistic fervor of the constituent parts of a system or the order-loving demon of Maxwell. Certain it is that even the best demon needs the individual molecule's contribution: the momentum of each corpuscle. Both have a share in creation and destruction.

II. TREATMENT

In recent years there is increasing awareness of the fact that the therapeutic process, the main source of all our dynamic knowledge of personality, has not yet been adequately observed in its totality. Hence we do not yet understand precisely the psychodynamics of the treatment procedure. To understand a process, it must be observed. This simple dogma which is the credo of science in the last 300 years has sufficiently proved its validity. It appears, indeed, a banal statement, and yet physics began as a science with the actual systematic observation of motion. Before Galileo, philosophers such as Parmenides, Heraklites and Aristotle talked about stability, motion and change, but never subjected to a methodical observation the things of which they were talking. The same is true for anatomy, which began when the dissection of the human body replaced idle speculations about its construction. And what is for us of greatest interest, the knowledge of personality, began with the banal, yet most significant, historical fact that Freud decided to listen patiently to his patients' complaints, something which strangely enough never was done before in a methodical fashion. All this shows that people seem to have a deep aversion to observe the phenomena they are curious about; they prefer to speculate, or what is worse, to talk about them.

One cannot, however, accuse psychoanalysts of neglecting observation. True, we too talk a great deal about things we did not observe directly—instincts, for example—but certainly the theory of the therapeutic process is based on a most extensive accumulation of observational data. We never really observed, however, the therapeutic process itself in its entirety. We have observed the patient over and over again, but not the therapist in action. We had to be satisfied with the highly selected and certainly anything but objective material, which the therapist could offer us concerning his own activities, reactions and feelings during treatment. In my belief, the next advancement in the realistic understanding of the therapeutic process will come from an approach in which both therapist and patient in their interaction will be studied by a group of trained observers.

I must hasten to state that the impetus for this type of study, which I am at present conducting, came to me first from my contact with David Shakow and soon afterwards from a few psychologists and psychoanalysts including Drs. Hilgard and Gill.

What can we hope to learn from this type of investigation? To outline these expectations first I shall have to turn to our current notion of the therapeutic process.

I shall try to reduce the theory of treatment to a few basic propositions, from which an integrated basic therapeutic model, or as we shall see, some alternative models can be constructed. This makes unavoidable repeating briefly our current notions on this subject, although I do not necessarily subscribe to all of them.

Theorem 1

During therapy, unconscious (repressed) material becomes conscious. This increases

the action radius of the conscious ego: the ego becomes cognizant of unconscious impulses and thus able to coordinate (integrate) the latter with the rest of conscious content. As a result, the ego can modify hitherto unconscious impulses so that it can include them into conscious voluntary behavior without internal conflict.

This theorem is based on our notions about ego functions. By internal perceptions the ego registers internal disturbances of physiochemical equilibrium, perceiving them as needs and sensations; by external sensory perceptions it registers the conditions existing in the environment upon which the gratification of internal needs depends. Through its cognitive function it reconciles these two kinds of data with each other and finally, as the center of motor control, it executes cognitive decisions in the form of coordinated voluntary behavior.

While these four ego functions—internal, external perception, integration and execution—are usually explicitly recognized, the importance of one component of the integrative act is somewhat neglected, namely, the omission of perceptory data. An adequate amount of loss of information, to use a term current in communication theory, is a necessary part of the integrative act as well as of the conduction of messages. Otherwise, the communication channels would be hopelessly jammed, and the coordinating function would become impossible by an excessive load of useless—at the moment useless—information. In our empirical era this point cannot be overemphasized. Many "research projects" die from the plethora of indiscriminately collected data. This is due to the erroneous belief that in a "really scientific" study everything must be registered and correlated with everything else. It is quite true that in both the theory of repression and of apperception the principle of loss of information is implicit. Repression is defined as excluding from awareness material with which the ego cannot deal at the moment. It is apt to become a pathological phenomenon, because the loss of information is emotionally conditioned, and therefore valuable information is lost. And similarly, apperception consists in the transformation of the crude data of perception into a meaningful Gestalt which does not contain everything which was perceived. A similar process Freud described by his analogy of the spotlight of attention turned upon readily available but not consciously registered material, through which preconscious material becomes conscious.

Theorem 2

According to the basic model, mobilization of unconscious material, eventually leading to its becoming conscious, is achieved mainly by two therapeutic factors: (a) free association and (b) the patient's emotional interpersonal experiences in the therapeutic situation (transference). The therapist's objective, non-evaluative, impersonal attitude is the principle factor in mobilizing unconscious material. This objective, impersonal, non-evaluative attitude is the essence of the blank screen concept and is the core of the basic model.

Theorem 2 is based both on the *theory of free association* and the *theory of transference*. We shall discuss them separately.

The theory of free association. It is assumed that as a result of the patient's eliminating conscious control over his train of thoughts, preconscious and eventually unconscious material appear in his consciousness. This expectation is based on the assumption that there is an ever-present tendency of repressed material to enter into consciousness, which tendency is counteracted by resistance emanating from the ego. This assumption is consistent with the view that the ego is the central controlling agent of voluntary goal-directed behavior and therefore every impulse which seeks expression necessarily seeks admittance to this central executive agency.

According to the basic model, the technical device of free association requires the patient's full conscious cooperation. The so-called basic rule to abandon conscious control over trains of thought and to tell without any distortion everything at the moment it occurs is explained to the patient before the analytic treatment begins. Moreover, the therapist explains to the patient the rationale of the basic rule. This way the pa-

tient is made a partner in the therapeutic venture, and from the beginning shares the responsibility for the therapeutic work which is a teamwork between patient and therapist. Without such cooperation between the patient's conscious ego and the therapist, psychoanalytic treatment is not possible. This precondition serves as the criterion for selection of patients who are suited for analytic treatment.

The theory of transference. It is only natural that the neurotic patient will sooner or later direct his typical neurotic attitude toward his therapist. In psychoanalysis this repetition of neurotic reactions is favored by the therapist's encouraging the patient to be himself as much as he can during the interviews. Free association is by no means merely an intellectual procedure for the patient, because he is increasingly encouraged to express freely repressed emotions and turn them toward the analyst.

According to the basic model, the therapist's objective non-evaluation attitude is the main factor in mobilizing unconscious material during the process of free association. At the same time, it facilitates the manifestation of transference.

It is important to keep in mind that the most significant repressions took place as a result of parental influences. In the course of analytic treatment, because of the prevailing permissive objective atmosphere, this material which has been repressed, or at least checked by inhibitions, is encouraged free expression.

The expression of unconscious material, however, does not take place in a vacuum. The mobilized unconscious tendencies of the patient become expressed in an interpersonal situation; the analyst becomes the object of the hitherto repressed tendencies. This is what is called transference. By its definition the transference is a regressive phenomenon. At the same time, according to the prevailing view, the transference is an ever-changing phenomenon. With the mobilization and free expression of different layers of unconscious attitudes, the transference changes. Its content corresponds to previous attitudes of the patient. As these infantile patterns become revived during the therapeutic process and are expressed towards the analyst, the transference neurosis appears.

In the case of chronic neurosis, much of the original parent-child relationship has become internalized. The external struggle between child and parents, the expression of hostility and sexual impulses, of guilt, of expiation by suffering and punishment—all this complex interplay between the child and his environment has been transformed into an internal struggle. The parental images become incorporated as part of the personality, and the external battlefield of emotional interplay becomes transplaced into the internal arena of the personality. The emotional interplay between the child and parents becomes an internal conflict between differentiated attitudes within the personality. During psychoanalytic treatment the *intrapsychic conflict becomes again transformed back into its original interpersonal form as an interaction between the patient and the physician as a parent or sibling substitute in the transference situation.* Only after this externalization of the intrapsychic conflict has taken place and the transference neurosis has developed can the real therapeutic task be undertaken—the treatment and cure of the transference neurosis.

Theorem 3

The central therapeutic task consists in the resolution of the transference neurosis. In Freud's view the transference neurosis is a less intensive repetition of original neurotic patterns and consequently consists of infantile reactions but is experienced in relationship to the therapist. Its intensity is reduced because the transference emotions are reactions to previous situations and not to the actual patient-physician relation. The only actual relationship between the patient and doctor is the fact that the patient comes to the physician for help. When in the patient's mind the therapist assumes the role of the father, mother, or older or younger sibling, he is actually none of these, and the transference relationship reflects the distortions in the patient's fantasy of the real situation. The nonpsychotic patient whose ego retains its capacity to test reality

is aware of the imaginary character of the transference, and this serves to reduce its intensity. Infantile conflicts are viewed openly in the personal relations with the analyst and are faced by the adult ego. Originally, when they were first repressed, the child's weak ego could not cope with them. These quantitative factors, the stronger adult ego facing a weaker edition of the original conflicts, constitute according to Freud the rationale of the treatment.

According to the basic model then, the confrontation of the irrational repetitive predetermined transference reactions (predetermined by the patient's earlier development) with the realistic interpersonal situation in treatment is the principal therapeutic agent in psychoanalysis. This confrontation is a cognitive act leading to insight, but must take place when the patient actually experiences his repetitive reactions in relationship to the therapist. Giving insight into what the patient actually experiences during treatment is called *emotional* insight. To give such emotional insight is the actual accomplishment of the therapist during the whole treatment procedure. Emotional insight, however, pertains not only to the content of the repetitive patterns, but includes also the patient's resistance. Interpretations of the patient's resistance against this type of insight are a major function of the analyst.

According to the basic model, this type of emotional insight resulting from the confrontation of transference patterns with the past and current reality situations is sufficient in itself to accomplish the therapeutic task. *It requires the revival of repressed memories of events in which the transference patterns originated.*

Ferenczi and Rank (9) introduced a modification of Theorem 3, maintaining that the repetition of earlier childhood patterns in the transference situation and their interpretation alone suffices to accomplish the therapeutic task, without reviving the memory of those previous situations in which the transference patterns originated. This theory stressed the significance of the emotional experiences in the transference situation, and did not consider essential the full understanding of their early genetic basis. The main therapeutic agent consists in the interpretation of transference reactions without necessarily connecting them with past memories. This modification never gained full acceptance, although its validity was never rigorously tested.

These three theorems are the foundation of the basic model of treatment. They represent a condensed summary of Freud's views on psychoanalytic treatment as he proposed them in his technical papers published between 1912 and 1915 (15, 16, 17, 18, 21). These three theorems constitute the basic principles of psychoanalytic therapy which fundamentally remained the same up to the present date.

This model disregards, however, two important facts. To begin with, the transference neurosis is not projected on a blank screen on which only the outlines of the objective therapeutic situations are visible, but on a screen with a design of its own which favors certain interpersonal events and which differs in each treatment according to the specific personality makeup of the therapist. The latter determines his spontaneous unconscious countertransference reactions and to a degree also his conscious reactions, which are determined not only by his theoretical preparation and practical experience in therapy, but also by his manifest personality traits. Furthermore, the interpersonal relationship in therapy is also influenced by the daily variation of the therapist's subjective state of mind, which is determined by the current experiences in his life. According to the blank screen concept, the analyst functions only as an abstract intellect who helps the patient's insight and serves as an impersonal target of the patient's transference reactions. Although the basic model did not exclude explicitly the influence of the analyst's distinct personality upon the interpersonal relationship in therapy, it did not account for this influence in detail. Freud as early as 1910 speaks about countertransference. The far-reaching consequences of this phenomenon, however, were never systematically studied (12). The other factor which the basic model disregards is that the therapist actually does

more than simply make the patient recognize the inadequateness, the past-determined, and not present-reality-determined nature of his neurotic patterns. He may introduce new value systems by suggestion (re-education) or simply by the fact of what he is as a person. In other words, he offers new identification possibilities to the patient.

I have to apologize for imposing on the reader's patience in outlining the basic assumptions on which the classical therapeutic model is based. My excuse is that I tried to reduce the verbosity with which these views are habitually presented, but a brief statement of these theoretical abstractions is unavoidable if we want to delineate the unexplored areas. This we can do by establishing those points where the theoretical model deviates from the actual events of the therapeutic process. Before trying to identify further the obvious shortcomings of the basic model, a few words should be said about important variations of the basic model as formulated by different authors, who sensed these inadequacies.

Many authors emphasized the significance of the patient's identification with the analyst as an important non-cognitive factor in bringing about a change in the patient's feeling patterns and behavior. This was perhaps most explicitly stated by Strachey in 1934 (29).

The essence of this view is that the analyst serves as a new superego inasmuch as the patient identifies himself with the analyst's attitudes. Interpretations are not only effective by convincing the patient intellectually, but also because the patient adopts wholesale—not because he became convinced by reason—the therapist's attitude, whom he trusts and admires. Freud was aware of this component of the treatment when he admitted the role of suggestion.

Another variation of the basic model appears in Rado's adaptive technique (24, 25, 26). He stresses that the therapeutic process to be successful must include more than helping unconscious material to become conscious by the standard procedure; it should also consist in the therapist's helping the patient to readapt himself to his actual life situation by a kind of active reeducational "emotional" process. This emphasis implies, as the identification theory, a not purely cognitive factor. "Insight alone has little if any therapeutic effect." The patient . . . "can learn to change his faulty emotional patterns in one way only: by practice; he must begin to do this before the eyes of the physician." Rado stresses the principle, as French and I did in previous writings (6), that the exploration of the past is not an aim in itself, but must be used for the understanding of the current difficulties in life from which the patient has retreated into his neurotic patterns, which are determined by his past experiences. Rado's criticism of the "parentifying type of psychoanalysis," however, is not a criticism of the accepted theory of treatment, but of its misinterpretation and abuses. The constant confrontation of the transference patterns, with the realistic physician-patient relation, as well as current life situations, is an accepted principle of standard analysis. On the other hand, Rado's admonition that the regressive evasion of the present should be constantly curbed and the patient must be brought back to "the adult level" is identical with my contention that regressive material is not always useful therapeutically. When the patient regresses emotionally to the pre-traumatic phases of his life, this must be considered as resistance against working through the pathogenic conflicts; it must be evaluated as a sign of resistance, and accordingly must be counteracted (4). Essentially Rado's contribution consists of his emphasis that the aim of the treatment is not the exploration of the past, but readjustment to the current problems of life which requires an active collaboration between therapist and patient.

Finally a third variation of the basic model is the theory of corrective emotional experiences which I have proposed and elaborated in recent years (2, 5, 6). This model requires the introduction of Theorem 4.

Theorem 4

The essence of the therapeutic process consists of the difference between the physician's reaction and that of the parents,

parent substitutes and/or siblings. The objective non-evaluating attitude which has the emotional connotation of helpful interest is certainly a most significant factor in allowing the emergence of repressed material and its revival in the transference situation. Equally important, however, is the recognition on the patient's part that these reactions are not suited to the analyst's reactions, not only because he is objective but also *because he is what he is, a person in his own right.* They are not suited to the situation between patient and therapist, and they are equally unsuited to the patient's current interpersonal relationships in his daily life. The patient is no longer a child, and the persons with whom he has to do are not parental or fraternal figures. This recognition is not merely intellectual insight, but is at the same time an emotional experience. Simultaneously recognizing and experiencing this discrepancy between the transference feelings directed towards the original objects in the past, and the reactions of the therapist, who is a distinct personality and is experienced as such by the patient, is what I called the corrective emotional experience (2, 5, 6). I shall briefly deal later with some technical conclusions derived from this fact.

This formulation, as also the two above-mentioned variations, do not require, however, *essentially* different therapeutic procedures. They spell out therapeutic factors which actually are operative in the usual psychoanalytic procedure, but are not explicitly stated in the original model. So far as Strachey's position is concerned, no one would deny the fact that identification with the analyst in addition to conviction arrived at by a cognitive process plays at least an intermediary role in every treatment. The patient accepts at first interpretations not only like geometrical deductions through becoming convinced by his own insight of their validity, but at first he accepts them because the analyst says so, knowing that the analyst is an expert with specific preparation and knowledge and also because of his positive transference. And as to Rado's model, no one would deny that most therapists—whether the text book prescribes it or not—apart from assuming the role of a detached intellect, also approach the patient's current life problems with a kind of collaborative reasoning and help him to draw some practical conclusions from the insight gained during the treatment. And finally, concerning the theory of corrective experience, it is hard to question the corrective influence of the fact that the analyst not only does not evaluate but he *is* a concrete person, different from the parental images toward whom the patient's transference reactions are not suited (non-adaptive).

The theory of the corrective emotional experience only spells out explicitly a factor which is an unavoidable component of every treatment.

According to this view, the fundamental therapeutic factor consists in *transference experiences* which are suitable to undo the pathogenic experiences of the past. In order to give these new experiences a corrective value, they must take place under certain highly specific conditions. How to establish these conditions is the main technical problem of the psychoanalytic treatment. According to the basic model, the desirable emotional climate of the treatment situation is achieved by the non-evaluative objective and impersonal attitude of the therapist. The basic model emphasized the patient's emotional insight into *the similarity* between transference reactions and the original infantile patterns. We add now the emphasis on experiencing emotionally the discrepancy between transference reactions and the analyst's actual behavior and personality.

In most chronic cases the re-experiencing of the injurious interpersonal relationships of the past under more favorable conditions, e.g., in the transference situations, is not alone sufficient. The patient must also obtain an intellectual grasp and recognize the past sense and the present incongruity of his habitual emotional patterns. The relative significance of emotional experience versus intellectual grasp is probably the most difficult and most controversial part of psychoanalytic treatment.

We may complete now Freud's formula-

tion of the therapeutic function of the transference.

The new settlement of an old unresolved conflict in the transference situation becomes possible not only because the intensity of the transference conflict is less than that of the original conflict, but also because the therapist's actual responses to the patient's emotional expressions are quite different from the original treatment of the child by the parents. This difference is twofold. First of all, the therapist's attitude is understanding and at the same time emotionally detached. The second factor is that he is actually a different person who does not elicit the same reactions as the original persons in the patient's life did. The important part in this view is that it becomes necessary for the patient to abandon or modify his old patterns. This becomes necessary because the therapist's reactions are different from those early responses on the part of the members of the family. The ego's basic function is precisely this: adjustment to the actual prevailing conditions. As soon as the old neurotic patterns are revived and brought into the realm of consciousness, the ego is challenged to readjust them to the changed external and internal conditions. This is the essence of the corrective influence of those series of experiences which constitute the transference experienced toward a person, who otherwise, if it were not for the distortion resulting from transference, would elicit quite different responses in the patient. There is no disagreement that this is the essence of psychoanalysis, although the personality factor was not explicitly stated in it.

From all the three variations of the therapeutic model, certain technical conclusions follow. Strachey did not draw technical conclusions from his emphasis upon identification. The obvious conclusion, however, is that no matter how powerful a weapon the identification with the analyst may be, the ideal goal remains gradually to transform identification into conviction based on real insight. The question remains, however, unanswered: how does genuine insight make the patient change his ways? Rado's emphasis on the therapist's active help in readjustment and my emphasis on corrective emotional experience are attempts to answer the well-nigh universal question which some time during his treatment every patient asks: "Well, I know now what is the background of my disturbance. How does this knowledge help me to change myself?" Rado takes up this challenge and helps the patient actively to apply insight to those actual problems of his actual life from which the patient withdrew to neurotic symptoms or neurotic behavior. No one would deny the value of such a procedure which to some degree, particularly at the end phase of treatment, is used by most experienced therapists.

The theory of corrective emotional experience leads to still another technical conclusion. This concerns the most opaque (in my opinion) area of psychoanalysis, the question of the therapist's influence on the treatment process by the virtue of being what he is: an individual personality, distinct from all other therapists. The evaluation of this most elusive element in the therapeutic equation is at present quite beyond our ken. We know only that the blank screen model is an abstraction, which is too far removed from the actual events during treatment. The therapist, himself a participant in the process, cannot properly evaluate the influence of his own personality. The supervisory situation gives us some vague notions. We observe that certain candidates handle certain types of patients better than others. We have no systematic knowledge, however, about this factor, not even a general hypothesis.

The concept of corrective emotional experience, however, allows some probable assumptions, which are supported by clinical observations. If it is true that the analytic relationship is a unique one, dissimilar to all previous experiences of the patient, and that this dissimilarity is the most central therapeutic agent, certain conclusions become cogent. What does this dissimilarity consist of? First of all, there is no known human relation with an equal amount of objectivity and lack of emotional involvement, a relationship which on the therapist's part consists only in the wish

to understand and help. This ideal, however, is never completely realized as the much discussed phenomenon of countertransference clearly demonstrates. The assumption that the well-analyzed analyst recognizes and controls his own emotional involvement and thus approaches the ideal of a blank screen—a detached intellect communicating with an impersonal voice—is never fully realized. This can be best illustrated by an experiment of thought (Gedanken-experiment): to visualize a treatment in which the patient never meets his therapist as a concrete person, but is received by a microphone through which the therapist is talking with a sound-filtered voice. That such an attempt never was made may indicate that with our practical or intuitive sense we never fully believed in the desirability of the blank screen even if it could be realized. It clearly shows that although our theory does not account for it, we recognize that the therapeutic situation is a highly personal one, in which two unique personalities interact and not as the original model assumes, one real person, the patient, with the therapist, a depersonalized intellect who has mastered psychoanalytic theory. The discrepancy between the patient's past interpersonal experiences, which he repeats in the transference, and the therapeutic situation is accordingly not only due to the objectivity and relative uninvolvement of the therapist, but also to the fact that he is a person in his own right.

A number of clinical examples I described in other writings showed that the "corrective"—or "reconstructive"—value of the therapeutic experience, is enhanced if the analyst's spontaneous reactions, the specific nature of his countertransference attitudes, or his studied attitudes are quite different from the original parental responses (2, 5, 6). Conversely, if the past parental attitude resembles that of the therapist, an interminable treatment results. The reason for this is quite transparent. The neurotic patters are adaptive, though unsuccessful, responses to the interpersonal experiences in the family. It is to be expected that if the therapist's countertransference reactions resemble—not, of course, in intensity but in quality—those which contributed to the formation of the neurotic patterns, the patient will continue to use them. Toward a doting father the son may respond with guilt, and consequently has to repress or project his hostilities. A paternal warm countertransference of the therapist will perpetuate this pattern, while a more detached therapist who has not quite as warm a response to the same patient would create a therapeutic climate in which free expression of the patient's negative feelings become possible and their repression or projection is no longer necessary. Moreover, the patient will recognize more readily the inappropriateness of his own transference reactions and will feel the need to replace them with more appropriate ones.

I do not need to elaborate this principle further, although I am fully aware of the vagueness of this type of observation and reasoning. My point is that this factor—the influence of the therapist's own personality upon the course and outcome of the treatment—requires further investigation. Neither is this the occasion to discuss in detail my previously made technical suggestions concerning the possibility that the therapist can do more than recognize, control and reduce the effects of spontaneous countertransference attitudes, in case they happen to be similar to those of the patient's parents or other members of his family, but he can replace them by purposefully lending a favorable kind of emotional climate to the interviews (2, 5, 6). I fully recognize the fact that the analyst cannot change himself and not every analyst is a good enough actor to create convincingly an atmosphere he wants. Here seem to lie the limitations of our technique, and we may have to accept the fact that every therapist is better suited to one type of patient than to others. Yet, I am convinced as a result of many years of experience that by planful behavior the therapist can considerably enhance his repertoire and can have good results even in cases in which his spontaneous reactions would not have a corrective value.

In a current investigation our team in the Mount Sinai Hospital in Los Angeles tried to formulate those questions which appear

most unexplored in the therapeutic process, in the first place this personal equation. We developed a procedure of observation and processing of data, from which we expect to obtain some clarifications of these opaque areas.

The main questions we ask can be listed as follows:

Is it true, and if so to what degree, that the patient repeats previously established reaction patterns toward the analyst?

Is it true that the patient's neurotic reaction patterns change as a result of the fact that he intellectually recognizes and emotionally experiences that these patterns had a meaning in the past (were reactions to family members) but are not adequate reactions to the analyst and his current life situations?

What is the significance of the repetition of such insights and experiences during a prolonged treatment? (working through).

Is the change in the patient's reaction patterns also influenced by the specific personality of the analyst?

Another unexplored area pertains to what is called psychoanalytically oriented psychotherapy.

Can psychoanalytic psychotherapy be defined as a method which in certain quantitative aspects differs from psychoanalysis proper, but fundamentally elicits similar psychological processes in the patient as does psychoanalysis?

What other factors enter into psychoanalytically oriented psychotherapy?

Is indentification with the analyst equally significant in psychotherapy as it is in psychoanalysis?

Are the direct educational influences of the analyst more important in psychotherapy than in psychoanalysis?

How do concurrent extratherapy events in the patient's life influence the therapeutic process, both in analysis and psychotherapy?

And more specifically, what is the relation of therapeutic experiences to life experiences in the course of the treatment?

Does therapy facilitate certain life experiences which in turn have effect on the therapeutic process?

For example, is a patient who becomes less inhibited as a result of treatment more apt to get involved in an affair, which may influence beneficially his development?

In which way should such a reciprocal effect between therapy and life experiences be included in the technique of treatment?

What is the role of pure chance? (Occurrence of therapeutically favorable or unfavorable life experiences.)

The essence of our investigative procedure consists in the direct observation and recording of the therapeutic process and a carefully planned processing of these data.

The therapeutic process is recorded and observed through a one-way mirror arrangement by three psychoanalytically trained observers who dictate into a microphone on the spot or after the interview their observations which are clustered in preestablished categories, such as content of the verbal exchange, non-verbal communications, transference manifestations, past and current events in relation to each other, dreams, manifestations of resistance, etc. One of the observers serves as audio-visual observer; the two others focus on non-verbal communication, the one on expressive movements, the other on the tonal qualities of speech. In addition, the therapist has a signaling device by which he can register five kinds of observations about himself, whenever they spontaneously catch his attention. These are: bodily sensations, feelings about the patient, wandering away of his thoughts to his own affairs, the sensation of suddenly recognizing some connections in the patient's material, and finally his thinking of the observers. All the material coming from the therapist and on the spot dictations of the three observers are synchronized on a tape. Also, certain physiological responses, such as sum total of body movements, pulse rate, skin temperature and electric skin resistance, are recorded both in therapist and patient. After the interview the whole team independently dictate their evaluations of the hour according to a prepared structured pattern. These synchronized records are finally studied by the coordinators who try to reconstruct all occurrences of the interview as an integrated

whole. There is no feedback from observers to the therapist, who had no access to any other material than the records of the interviews and his own post-interview dictations.

We try by this procedure to record every visible and audible event, and also the analyst's own thought processes. Yet the whole study is focussed around the appraisal of the influence of the analyst's personality upon the process.

Our principal questions are:

How does the analyst's individual personality influence the therapeutic process and how should this factor be included into a revised theoretical model?

And how does the analyst's theoretical orientation influence the therapeutic process?

These two questions necessitate that every member of the team, both therapists and observers, submit himself to preliminary tests and also prepare self-appraisals concerning his own values and attitudes and his own theoretical views about psychoanalytic therapy.

Finally, we wish to throw some light upon the recently so much belabored question:

What are the differences and similarities between psychoanalysis proper and psychoanalytically oriented psychotherapy?

Our pilot experiments already indicate that a valuable and hitherto neglected source of information about the affective interplay between therapist and patient consists in the observation of non-verbal communication.

One example. In our pilot study a female patient during an interview, by provocatively expressing her doubts of my interpretations, induced me to become more explicit and emphatic. After the interview I read the remarks of the observers which they dictated on the spot. This is what one of them reported: "analyst picked up pencil and is pointing it directly at the patient. Patient sticks her right index finger in her ear. After a while the analyst became less emphatic and smiled. Soon the patient took her finger out of her ear. Then the analyst began to point the pencil again towards the patient as he was explaining something to her. The patient now closes the top of her blouse". I was completely unaware of this non-verbal communication between the patient and me. Occasionally, of course, in everyday practice the therapist notices conspicuous non-verbal behavior in the patient, but seldom, if ever, recognizes it as a reaction to his own non-verbal behavior.

The main objective of this study is to learn more about the influence both of the therapist's personality and of his theoretical orientation upon the therapeutic process. To appraise the influence of the therapist's personality as distinct from his theoretical orientation, we are devising ways and means how to differentiate between the effect of the cognitive and non-cognitive—the experiential—components of the process. We shall have to observe at least two treatments conducted by therapists whose theoretical orientations and personality profiles have been previously established, and can be further reconstructed by the observers as the treatment progresses. This is facilitated by the device that the therapists, in their post-interview dictation, have to account for all their interventions.

We are keenly aware of the danger inherent in this type of research: namely, being swamped by an enormous amount of data. We know that we must avoid the compulsive urge to try to observe everything and try to correlate everything with everything else. The success of this type of study depends upon asking the correct questions and making a sensible selection of data to be processed.

Our main hope is that the device of exposing all therapeutic events to several trained persons—three observers and several coordinators—will allow us to subject the elusive material we are dealing with, if not to the same rigorous standards of scrutiny which prevail in the natural sciences, but at least to more objective control and more detailed work-up than is possible in the private practice of psychoanalysis.

This last reference to the prevailing research in psychoanalysis as it is conducted in private practice, should not be interpreted in a derogatory manner.

It is appropriate at this point to stop and to realize that practically all our psychodynamic knowledge was acquired in this

fashion, private practitioners making scattered observations, which never could be fully reproduced, studied and precisely communicated to others. And yet the results of their unsystematic studies revolutionized psyichiatry and the social sciences and produced the first comprehensive theory of personality. This accomplishment is most impressive. At the same time it is equally impressive that since the discovery of the role of transference and resistance as proposed in Freud's technical papers between 1912 and 1915, no great technical advances have been made. Whether the therapeutic effectiveness of the treatment has increased at all in all these years is hard to establish. Comparing this stagnation with the great advancements in organic medicine during the same period of time, the striking contrast cannot be simply ignored. The obvious explanation is that we continued to use the same methods of observation and became too complacent and therefore averse to try out new avenues of approach. Primarily this is due to the fact that psychoanalytic research remained outside of academic publicly supported institutions and continued to be carried out as an accessory of private practice. This explains at least partially the fact that we omitted to observe the totality of the therapeutic process. The intimate nature of the material was our excuse. No doubt privacy in matters of personality is even greater than the privacy of the body, and the difficulty of penetrating the privacy of the analyst is perhaps the greatest obstacle. In order to expose the whole therapeutic process to adequate objective observation by others, the therapist must be willing to expose himself. I shall not further elaborate this delicate subject. The analyst, unlike any other physician, has been in the enviable position of being able to expose from his activities only as much as he was willing to expose. I should like to add: even if he is willing to tell everything, he cannot tell what he is not aware of and cannot observe, namely, the manifestations of his own idiosyncratic qualities. Therefore, it appears to me not too exaggerated a hope that the mere fact that analysts are willing to expose their activities to the direct observation of their confreres will give opportunity to learn more about the therapeutic process.

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(Manuscript received March 12, 1958).

The principles of mental hygiene are as follows. Can you embrace the One? Can you never forsake the Tao? Can you divine fortune and misfortune without the help of soothsayers? Do you know when to stop? Can you let unimportant things go? Can you learn not to depend on others but to seek it in yourself? Can you come and go unfettered in spirit and can you purge your mind of knowledge? Can you be innocent like a newborn child? The baby cries all day and yet his voice never becomes hoarse; that is because he has not lost nature's harmony. The baby clenches his hands all day without holding anything; that is because he is following his original character. The baby looks at things all day without winking; that is because his eyes are not focused on any particular object. He goes without knowing where he is going, and stops without knowing what he is doing. He merges himself with the surroundings and moves along with it. These are the principles of mental hygiene.

—CHUANG TZU (c. 300 B.C.)

Information measurement has shown that language has a redundancy of about 50 per cent. It is suggested that behavior in general also has about 50 per cent redundancy—that it seeks an equilibrium between the unexpected and the predicted, between organization and disorganization. This equilibrium theory of redundancy may be useful for studying cultural and personal value systems.

BEHAVIORAL REDUNDANCY

by Benjamin N. Colby

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EQUILIBRIUMS of various kinds at work in the brain and measurable in electrical units will most likely be among the first stepping stones to more generally defined, scaled equilibrium principles of human behavior. Other contributions in this direction may come when the intricacies of endocrinology and other biological homeostatic tendencies can be linked adequately to outside environmental events in scaled, quantitative terms. Before that time comes general equilibrium theories in the behavioral sciences can rarely be more than heuristic. But this does not deny the value of heuristic hypotheses, and it is the intention here to show that if a heuristic hypothesis of equilibrium can stand up in the field, promote new discoveries, and demonstrate a wide range of application, the call for quantification can be inappropriate if not unrealistic. This is illustrated in the following description of how an equilibrium theory of redundancy can be useful for studying cultural and personal value systems.

Man dislikes venturing away from the relatively safe order of the known into any sort of new order less understood at the outset. This is an underlying tendency of human behavior long recognized both generally and particularly. Clyde Kluckhohn pointed out the great extent of this tendency when he said, "Most men most of the time, dread both spontaneity and change in most of their activities" (2). Certainly the more regular, understandable, and predictable man's environment becomes, the more he is freed to do those things not strictly concerned with his mere animal existence. It is to his interest to avoid the disruption change may bring. Predictability of tomorrow's events and of what people may do or think is of paramount importance.

But there are other trends in the opposite direction which temper the dread of the unexpected. These are trends toward disorganization, toward the unknown, and toward experimental innovations. They have been discussed under many rubrics—the creative desire at one extreme and the destructive urge at the other. Freud's conception of the instincts, notably the death instincts can be included here.[1]

The formative process, the trend in man which moves toward organization usually predominates when this organization gives man power over his environment. But curiously enough the opposing, often irrational force which tends to destroy that organization and break up thought-structures does not run counter to man's ultimate objectives—preservation and comfort. It is vital to them. When man loses control over some part of his environment, he has to seek new methods and organizations to readapt himself. Before this is possible the old habits and organizations which now are useless must be eliminated. Here is where the de-

[1] Freud considered the chief objective of the instincts to be the release from mental and physiological tensions. Logical inconsistencies were regarded as fusions between various life and death instincts or their derivatives. Avoidance of these inconsistencies may be possible through a breakdown and realignment of Freud's opposing forces into behavioral redundancy terms.

structive force functions. Without it, man's methods of environmental control would be too rigid, his freedom of choice would be dangerously restricted and the feedback mechanism so necessary to adjustment would be paralyzed. For predictability to be constantly maintained at a high level of efficiency, value systems must be changeable, must always be in process. Man's inner disorganizing force thus is always at work, even to some extent in periods of easy living, when the environment is adequately controlled. By maintaining some sort of equilibrium between order and disorder or rigidity and fluidity he can stay at peak adjustment. Such is the maturation process in culture and in personality.

In addition to the functional working of these basic tendencies there appears to be a complementary balancing mechanism which responds in a reverse manner to environmental circumstances. Simply stated, in times of easy, predictable living man may seek out the unexpected as a kind of relief from boredom; while in times of chaos man may flee from it, grasping at the slightest remaining straw of certitude before making any creative effort to reformulate the problems besetting him.

The process of constant reformulation, reassessment, and affirmation of value structures regulated by the movement either toward or away from organization does not always result in increased accuracy—at least by standards of science. Some value systems have to meet needs other than environmental exigencies. Such is the case when man seeks a cosmic or religious "understanding." Realism in myths and rituals, for example, is often only an ancillary criterion. But in spite of empirical inconsistencies, myths and rituals provide order and form in those regions of human concern otherwise most disordered and bewildering. In this area particularly, the formative process moves at a slow rate and thought-structure is rather rigid.

When man is not faced with the exigencies of his environment—as is the case in sleep, or other situations provoking imaginal productions—structure yields its rigidity and there is greater spontaneity in mental process. Experiences of unrelated endogenous images represent man's extreme penetration point of that disordered, unstructured world in which it is impossible to remain for greatly prolonged periods.

Summarizing thus far the processes involving myth, ritual, and endogenous images are probably linked with homeostatic or equilibrium systems from which the balance between order and disorder discussed in this paper has been inferred.[2]

Analogies are fraught with difficulties, particularly in the behavioral sciences; but there is one special example from linguistic and communication studies that should be mentioned here, for it may have closer connections to our problem than those of mere analogy. It has been found that languages have about 50 per cent redundancy—where half the phonemes in a message are the result of free choice and half of them are determined by the statistical structure of the language. Greenberg, Osgood, and Saporta (1) have shown that over a period of time, though the redundancy of language varies slightly, it always remains near 50 per cent. Using the material of Llorach (7) the redundancy of Spanish was calculated for four periods in Spanish linguistic history. The results from classic Latin through to Modern Spanish were respectively, 51.2, 43.7, 52.5 and 49.1 per cent. The process resembles the swinging of a pendulum or the maintenance of an equilibrium centering on 50 per cent redundancy.

Direct connections between this phenomenon and other types of human behavior have not been established. But for all present purposes and with the exception of the time dimension, it is perhaps easier

[2] If this principle is as basic as it seems, one might suspect that it is not restricted to humans alone. Recent studies of rat behavior suggest this. D. Krech (4) at the University of California has demonstrated how a high level of cholinesterase in the neural system seems to cause spatial behavior to predominate in rats and a low level of it to cause visual behavior. There is a decline of cholinesterase with age which also correlates with a high amount of visual behavior in the animals. Visual behavior is more stimulus-bound and represents a more rigid pattern of action. This may indicate a tendency towards greater general redundancy with age.

to think in terms of such a principle. *One may say then that life in general, like language, seeks an equilibrium of about 50 per cent redundancy—an equilibrium between the new (unexpected) and the old (predictable); between disorganization and organization.* Without some sort of homeostatic or "pendulum" principle in which deviations from the midpoint are checked and reversed before getting out of hand in either direction, life would not be possible. Only by maintaining the proper equilibrium can growth occur.

Refining this somewhat, the two concepts of organization and disorganization can each be subdivided into an active and passive aspect. The active aspect of organization is man's striving to create form. The passive aspect is his desire to repose in that form which has already been created. These two aspects are similar to such dichotomies as creativity and culture, or freedom and determinism. Man's desire to repose in that form which has already been created, the passive aspect, is merely the desire of all men for a stable culture and society, in which there is a minimum of new and unexpected things. On the other hand, the active, creative desire is his rebellion against his culture—in its confining elements.

Moving to the active and passive aspects of "disorganization," one encounters a more difficult position. Perhaps the various types of schizophrenia represent these two aspects in disequilibrium once they have moved too far from the mid-position. In the "adjusted" individual, the active aspect of disorganization is the disorganization which takes place prior to a better and more adjusted organization. The passive aspect of disorganization may represent an innate need for novelty. An input of new, unexpected information probably functions to prevent the atrophy of adjustment mechanisms within the individual.

Proceeding to a more specific level of discussion, it will be recalled that in imaginal productions of the human mind unrelated, endogenous images were described as representative of a low redundancy situation, while imagination in a mythical or ritual context was considered to be highly redundant. The relationships are actually not nearly so simple. Imaginal productions involve a whole network of relations of entirely different qualities. One of the most important of these relationships is that between the mind and kinesthetic inputs. Recent experiments by Lilly and by Hebb (6) have shown that the relative presence or absence of physical stimulation has a direct bearing on the image-producing processes within the brain. At McGill University subjects cut off from sensory stimulation have found it difficult to carry on organized, directed thinking for any sustained period. Experiments by Lilly at Bethesda have shown the same thing. After prolonged absence of sensory stimulation mental patterns apparently move from random thoughts and images to fullflown three-dimensional visual phenomena.

These developments point to an inner biological need. They reflect important relationships between imaginary or symbolic processes (whether of a fantastic or rational nature) and the sensory physiology of the body. It is quite possible that the physiological states or conditions which must be maintained depend in part on those processes which in humans have developed into imaginal and symbolic functions.[3]

In view of these considerations, the work on human value orientations being carried out by Charles Morris (8) has a very special interest. Morris has been working with 13 one-paragraph descriptions of value orientations or "Ways of Life" which are similar to philosophical or religious attitudes. Way number One is a description of the Apollonian

[3] The links between imaginal and symbolic processes at one level and the clustering of granules in neurone cells, for example, at another, are of course tremendously difficult to establish. Intermediate to such a task lie the mechanisms of energy liberation; the maintenance of consciousness levels; the instinctual mechanisms which cause the initial imitation of speech sound; and the subsequent reorganization of mental patterns once symbol and object have been connected in the mind. If we wish to study the relationships of the different aspects or levels of human behavior from the cell and the organ to the individual and the society, the connections between language and physiology are important as an integral part of such an understanding.

orientation to life, Way number Three, the Christian, number Seven, the Maitreyan, and so on. These thirteen descriptions are read by subjects and ranked according to the subject's order of preference. At an early stage in his analysis, Morris placed his value orientations into three basic categories, and emphasis on sensory or external stimulation can be seen to have an important part in at least two of them:

> Dependence was characterized as a need "for easy compliance with the world. The person depends, hangs on the physical and social world, is receptive to it, belongs to it, does not retreat from it or attempt to make it over, wishes sustenance, wishes a dependable world." Dominance was put in terms of "the need to be dominant in a situation (which is not necessarily to need to be domineering). Not a dependable world but a controllable world, a world in which effort is efficacious, a world in which one can initiate changes and lead them to a desired eventuation. The need is for power over persons and things, the excitement of overcoming, the sense of domination." Of detachment it was said: "It is a movement away from excessive external stimulation, away from demanding pushing world. A movement toward the inner man. Not comfort is wanted, not power, but awareness of oneself. Not involvement in the world, but the world at a safe distance. That one may listen to the self, protect the self, savor the self, live with heightened consciousness." (8, pp. 27–28).

The person with dependency leanings will place greater emphasis on sensory stimulation. He is bound to the outer world through the senses. On the other hand, introversion or detachment is due to dissatisfaction with the confining, redundant outside social world. As the introvert moves further from the mid-point of redundancy his actions tend to be of an increasingly mental nature providing release from tactual kinesthetic and other sensory stimulation. In Morris' words, it is "a movement toward the inner man." Dominance stresses, above all, action, and it is at the mid-point of behavioral redundancy that action is most predominant. This is the point toward which adjustment tends to concentrate, the adjustment process itself consisting primarily of the active aspects of organization and disorganization. Dominant people usually stress action, particularly if they have a strong physique and are high in social orientation.[4]

At this point we might ask, how is it that some individuals apparently crave a great deal of security through close adherence to cultural norms and commonplaces while others may tend toward a position of extreme idiosyncrasy or deviance, if some sort of middle position between the two is vitally necessary? The answer undoubtedly lies in the complexity and multidimensionality of the problem which we have up to now pictured as a simple, general equilibrium process. For expositional purposes we shall refer to the general model as that of "behavioral redundancy." Behavioral redundancy may function as a governor of a substantial number of more specific redundancy equilibriums which are weighted and balanced differently among individuals according to physiological and anatomical differences, as well as to differences in experiential history. A person may balance high redundancy from one area with low redundancy from another. A deviant in a social group may demonstrate little need for a high redundancy of behavior in culture and may compensate for this with some other type of redundancy not immediately apparent to others of his social groups.

The formulation of value systems is just one aspect of the formulation of redundancy systems, but as values provide a kind of programming for the individual or a code of conduct, it is easier to visualize behavioral redundancy in a value pattern framework. Returning to Morris' thirteen Ways of Life, it may be seen that these Ways must result either in a certain amount of redundancy, (expectedness or organization) for the individual, or in the means for him to create it. The following list of the thirteen Ways is our own characterization of them in the interest of brevity. Tentatively listed opposite each one is the type of redundancy or expectedness which it promises to provide:

[4] Dominance, however, can also mean self-control or strength of will power and may be entirely divorced from strong social affinities.

Way	*Expectedness (or means of control)*
1. Apollonian	Unchanged order
2. Independence	Inner order
3. Sympathy and love	Golden Rule
4. Dionysian	Conformance to social situation
5. Group participation, no privacy	Cohesion of the ever-present group
6. Practicality & action, master changing conditions	Effectiveness of practicality and action
7. Eclectic—action, enjoyment contemplation	Confidence in adaptability and method
8. Comfort—friends, food, relaxation	Catering of others, familiarity of surroundings
9. Receptivity, wait	Trust
10. Stoic control, denial of comfort	Inner order
11. Inner life, meditation	Power over imagination
12. Adventure, power manipulation, accomplishment	Self-confidence in abilities
13. Let oneself be used, devotion and confidence	Determinism

Value structures as we tend to see them now may not always balance at the midpoint of redundancy. They may intermesh with redundancy patterns in "traits" which are psychologically more peripheral, or "motives" which are more central and profound. The kind of redundancy involved must be defined. To see how the placement of values varies according to the kind of redundancy involved, the following examples will be useful: Referring to Morris' earlier categories of Dependence and Detachment, a continuum of social redundancy can be set up. In this continuum, the movement toward people and the establishment of rigid forms of social usage and roles represent the high redundancy end of the continuum, and the movement away from people represents the low end. Movement toward people would be Morris' "Dependence" and movement away from people would be his "Detachment." The relative or rank positions of the thirteen Ways on this continuum are shown in Figure 1.

Many different types of redundancy continua can be set up in this manner. On another level, and to introduce further

Detachment												Dependence
2	11	10	9	6	7	12	1	3	4	13	5	8
Low Redundancy										High Redundancy		

FIG. 1. An Ordinal Scale Showing Positions of The Thirteen Ways on a Continuum from Low to High Redundancy.

complexity, some of Morris' thirteen Ways allow more variation and experimentation with values themselves while others are more rigid. In Figure 2 these Ways are shown on a variation continuum:

Wide Variance												Narrow Variance
2	4	7	11	12	6	8	9	5	3	1	10	13
Low Redundancy										High Redundancy		

FIG. 2. An Ordinal Scale Showing Positions of the Thirteen Ways on a Continuum from Low Redundancy to High Redundancy According to the Degree of Variance from Rigid or Normal Values That Is Allowed by Each Way.

To introduce another dimension, the balancing of the redundancy pendulum cross-cuts the concepts of structure and process. A low redundancy in structure balances a high redundancy in process, and vice versa. A high effectiveness in methodology can supply needed self-assurance in the face of structural chaos, while a well-defined structural organization can provide a feeling of safety and assurance when effective methodology is lacking.

An illustration of very high structural redundancy and the consequent lack of redundancy in process can be found in the Trobrianders who are context-bound in cultural patterns of extreme rigidity. Dorothy Lee (5) says, "Within the pattern, the Trobriander feels safe and acts with assurance. Away from home, he likes to reproduce known previous order, even physically." On the other side, Lee's description of United States culture is an excellent illustration of redundancy in process: "for members of our culture, value lies ideally in change, in moving away from the established pattern; and safety is insured through scientific prediction, not exact experience."

It is of course possible for emphasis on redundancy of structure or process to approach extreme positions before adjustments take place. Even during unfavorable ecological conditions the disorganization or discarding of ineffective value patterns may be so strongly resisted as to endanger the cohesion of the group. Such has been the case with the Texan homesteaders described by Vogt (9). These Texans or "Okies" which

did not emigrate all the way to California as did the others, were probably more tenacious of their old value patterns which, in F. Kluckhohn's categories, especially emphasized orientations toward the future, mastery over nature, and individualism. Such orientations in behavioral redundancy terms stress process too highly and apparently are not an effective value combination for their particular situation. The "Homesteaders" are rapidly losing members from their settlement in New Mexico, and Vogt predicts the eventual disappearance of the community. It is interesting that a nearby Mormon town represents a highly successful adjustment which, with regard to the ecological circumstances, does not have such an apparently unbalanced emphasis on values of process.

The characterization of a culture by its position on a dimension of behavioral redundancy need not be along lines of structure and process alone. The problem may be approached from any number of directions. Generally speaking, it is not unreasonable to assume that the low redundancy area of a culture can be detected where there is a large degree of variation through time and situation. Values found in the low redundancy area of a culture, because of their variation, require a greater amount of conscious consideration by the members of that culture. Hence they tend to be more explicit than the other type which, due to high redundancy, provides the basis of common assumptions and understandings—the results of which may or may not be immediately evident to the outside observer. Those values which are less redundant in a culture thus tend to be values of the individual—values which he himself selects, sometimes consciously, sometimes not. These individual value selections can never be entirely free ones, for they must balance somehow with the more redundant, often more implicit, values of his culture.

Thus to arrive at the universal values which Kluckhohn (3) and others are concerned with, it may be profitable to study the range of idiosyncratic values found among individual members of a culture. If laws can be discovered which explain and predict the direction of change of these less redundant, idiosyncratic values over time and in various situations, one may find an important key to the study of the universal values against which these idiosyncratic values have to adjust or balance. This will undoubtedly be of crucial importance for predicting the direction of over-all cultural change. The concept of behavioral redundancy may be of use for this purpose.

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(Manuscript received September 24, 1957.)

Heider, Newcomb, Cartwright, Harary, and others have concerned themselves with the balance of small groups, including the smallest of all—the dyad. And Festinger has described related aspects of group interpersonal relations in terms of consonance and dissonance. This article, in similar mode, develops a model of the behavioral effects of social rewards on compliance with learned norms. Several hypotheses are derived and tested by determining relationships among 361 factory workers and their respective 51 foremen.

BEHAVIOR, NORMS, AND SOCIAL REWARDS IN A DYAD[1]

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THE relationship between actual behavior and norms has been an object of attention by the social psychologist, the student of personality and also by the therapist. It has been often stressed that harmony between the perceived self and the ideal self is of great relevance to the mental health of the individual. The ideal self is, in essence, a product of the socialization process, shaped by the primary groups in which the individual grows and socializes. Yet the ideal self of childhood may not fit the requirements of other groups encountered later in life. It is indeed recognized that differences in role expectation between members of such small groups as the family or a working team may lead to behavior problems and maladjustment (12).

According to psychoanalytic theory the ideal self is an introjection of the norms of the socializing agents. Compliance with these norms has been associated with reward since the early childhood period (2, p. 38). It seems, therefore, that the individual becomes conditioned: (a) to associate reward with compliance with the norm of the other; (b) to generalize from the norm of the other to the norm of the self and, therefore; (c) to associate reward with compliance with the norm of the self (13, p. 241). Thus compliance with norms seems to be associated with certain rewards in the socialized individual: the reward coming from compliance with his own norm and the reward expected from compliance with the norm of the other. These may be called *social* rewards to distinguish them from other kinds of rewards such as those based on the satisfaction of biological needs.

Later, when the individual comes in contact with other groups and acquires new roles, he is likely to discover that the norm of the other is sometimes different from his own. Then he has to choose between losing the reward associated with compliance with his own norm or losing the reward associated with compliance with the norm of the other. One of the possible ways for solving the conflict is through imitation (2, p. 92), i.e., the individual may choose the same solution which appears to be also chosen by the other.

The foregoing discussion of the relationship between behavior, norms and associated social rewards is relatively simple since it refers to the situation as perceived by one individual. Truly enough this individual has an image of the perception of behavior and of the norm of the other. Nevertheless the perception of the other, of the same situation, may be different. In order to obtain a complete view both pictures of the situation should be considered and compared. This is precisely what our formal

[1] This paper is an outgrowth of a pro[illegible]m of research in interpersonal relations in industry, supported in part by the Ford Foundation. The author is grateful to Drs. S. Eisenstadt, C. Frankenstein, and L. Guttman, all of the Hebrew University, for comments on a preliminary draft entitled "States of Interpersonal Behavior."

model attempts to do. The model is concerned with the relationship between norm and actual behavior. The assumption that compliance with norm is associated with social rewards is not essential to the formal development of the model. But, if such assumption is accepted, the model can be viewed as a system for the production and distribution of social rewards in the dyadic relationship. We turn now to the description of the model.

Let us consider two interacting individuals, a dyad. Each one of the two persons of the dyad may be regarded as performing three kinds of functions as follows:

1. acting, i.e., being an *actor*;
2. perceiving behavior (of himself or of the other), i.e., being an *observer*;
3. being ascribed a certain perception, i.e., being an *alias*.

These three functions take place on two different levels: the actual and the normative.

Each person can be actor, observer, and alias. In a dyad there are, therefore, two actors, two observers, and two aliases. The observer is a potential informer for the researcher. A given act is perceived by the two observers (the actor himself and the other) and ascribed, by each observer, to each one of the two aliases (the alias of the observer himself and that of the other). Through ascription to the other, the observer obtains a picture of the behavior of the actor, as apparently perceived by the other observer.

Ascription consists in assigning the perceived stimulus to a given type of behavior. It is assumed that types of behavior are exhaustive and mutually exclusive. That is, for every ascription there always exists one type of behavior, and one only, to which the stimulus can be assigned. An example of the construction of a list of types of behavior, appropriate for wide classes of social psychological problems, has been given in an earlier paper (5).

Two given ascriptions may or may not be assigned to the same type of behavior. This identification of two ascriptions with the same or different types of behavior may be relevant in certain cases or for certain research purposes, and irrelevant in certain other cases, or for other research purposes. For the purpose of the present model it is relevant to know whether an actual action by a given actor, perceived by a given observer, and ascribed to a given alias, does or does not belong to the same type of behavior as the corresponding norm. That is, two ascriptions are said to be *relevant* to each other when their levels are different (one actual and one normative), but the actor, observer and alias are the same. Otherwise the two ascriptions are said to be *irrelevant*.

Two relevant ascriptions are in a state of *consonance* when they belong to the same type of behavior. Otherwise they are in a state of *dissonance*. It is assumed that an action is rewarding when it is consonant with the norm and not rewarding when it is dissonant with it. This way of comparing the behavior types of different ascriptions may be regarded as a special case of Festinger's Theory of Dissonance (4). A clear definition of "relevance," for this special case, has been provided above.

CLASSIFICATION OF STATES

We shall use valence as a generic term for consonance and dissonance. Thus any pair of relevent ascriptions has a valence. A valence is characterized by a given actor, a given observer and a given alias. Consonant valence indicates reward; dissonant valence lack of reward. The reward specified by the valence is produced by the actor, received by the observer and attributed to the alias. For example, the valence for actor 1, observer 2, alias, 2, indicates the reward that observer 2 attributes to himself from the action of actor 1. The valence for actor 1, observer 2, alias 1 indicates the reward that observer 2, attributes to the other for his own action. It has been indeed suggested that an individual gives reward to himself when he behaves according to his own norm and gives reward to the other when he behaves according to the norm of the other.

Let us consider the two valences for a given actor, a given observer and the two aliases. These two valences indicate how the reward produced by the actor and received

by the observer is distributed between the two aliases. There are four mutually exclusive possibilities (see Table 1):

1. The observer perceives the action as consonant with both the norm of the actor and the norm he ascribes to the non-actor. This is the consonant, or C state. The action is seen as rewarding for both the actor and the non-actor.

2. The observer perceives the action as dissonant with the norm of the actor as well as with that of the non-actor. This is the dissonant, or D state. The action is not seen as rewarding neither for the actor, nor for the non-actor.

3. The observer perceives the action as consonant with the norm of the actor and dissonant with that of the non-actor. This is the actor-consonant, or A state. The action is seen as rewarding for the actor and not rewarding for the non-actor.

4. The observer perceives the action as dissonant with the norm of the actor and consonant with that of the non-actor. This is the other-consonant, or O state. The action is seen as not rewarding for the actor and rewarding for the non-actor.

Each one of these four states describes the valences of the two aliases as seen by a given observer with reference to a given actor. The first two states are *homovalent*: the same valence is attributed to the two aliases. The last two states are *heterovalent* a different valence is attributed to the two aliases. Homovalence indicates that the reward produced by the actor and received by the observer is equally distributed between the two aliases. Heterovalence indicates that, either because of differences in the ascribed perceptions of the action, or because of differences in the ascribed norms, or for both reasons, the action cannot produce reward for both aliases; reward for one person leads to lack of reward for the other. There exists, in heterovalent states, a problem of choice between the two rewards. In homovalent states the identity between the norm of the self and the norm of the other, established during childhood training, is maintained. In heterovalent states the individual is faced with a new conflicting situation.

TABLE 1

STATES OF A GIVEN OBSERVER FOR A GIVEN ACTOR

State	Description	Valence for actor	Valence for non-actor
C	Consonant	+	+
D	Dissonant	−	−
A	Actor—consonant	+	−
O	Other (non-actor)—consonant	−	+

Note. Consonance is indicated by +, dissonance by −.

Each state is characterized by a given actor and a given observer. In a dyad there are two actors and two observers and this makes a total of four (2×2) states. Namely: state for actor 1 and observer 1; state for actor 2 and observer 1; state for actor 1 and observer 2; state for actor 2 and observer 2. State for actor 1 and observer 1 and state for actor 2 and observer 1 constitute the *intrapersonal subsystem* of observer 1. The other two states constitute the intrapersonal subsystem of observer 2.

State for actor 1 and observer 1 and state for actor 1 and observer 2 constitute the *interpersonal subsystem* for actor 1. The other two states constitute the interpersonal subsystem for actor 2.

The intrapersonal subsystem.

The intrapersonal subsystem of a given observer gives a picture of the production and distribution of rewards of the dyadic system as seen by this observer. The rewards produced by the two actors of the subsystem are distributed by the observer between the two aliases. The states of the subsystem indicate the amount of reward received by each alias and its source. These states, and the corresponding pattern of valences are listed in table 2.

Table 2 shows whether the conflict between rewarding the self and rewarding the other is present for both, neither, or one actor only. When both states of the subsystem are homovalent there is no conflict for either actor. This happens in states *C C*, *D D*, *C D*, and *D C*. When both states are heterovalent the conflict occurs for both actors (states *A A*, *O O*, *A O*, and *O A*). In all the other cases one state is homovalent

TABLE 2
THE INTRAPERSONAL SUBSYSTEM OF OBSERVER 1

State		Valence Pattern			
		for actor 1		for actor 2	
for actor 1	for actor 2	alias 1	alias 2	alias 1	alias 2
C	*C*	+	+	+	+
D	*D*	−	−	−	−
C	*D*	+	+	−	−
D	*C*	−	−	+	+
A	*A*	+	−	−	+
O	*O*	−	+	+	−
A	*O*	+	−	+	−
O	*A*	−	+	−	+
C	*A*	+	+	−	+
C	*O*	+	+	+	−
D	*A*	−	−	−	+
D	*O*	−	−	+	−
A	*C*	+	−	+	+
A	*D*	+	−	−	−
O	*C*	−	+	+	+
O	*D*	−	+	−	−

and the other heterovalent and the conflict occurs for the actor in the heterovalent state only.

Table 2 also indicates whether or not the relationship between rewarding the other and being rewarded by him is preserved. In order to know this we need to observe the fourth and fifth columns of the table. When the two entries of these columns are of the same sign the relationship is preserved, otherwise it is not preserved. The relationship exists, for example, in state *C C*: here observer 1 perceives that his own action is rewarding for the alias of 2. At the same time the action of 2 is perceived as rewarding for the alias of 1. In state *D D* the relationship likewise exists, albeit in negative form: the observer perceives that his own action is not rewarding for the other and the action of the other is not rewarding for him. Also in states *A A*, *O O*, *C O*, *D A*, *A D*, and *O C* the relationship is found to exist. When there is conflict and it is solved in the same manner for both actors (states *A A* and *O O*) the relationship between reward given and reward obtained is preserved. In the first six states of Table 2 either there is no conflict for either actor or it is solved in the same manner for both actors. In all these cases the total reward attributed to one alias is equal to the total reward attributed to the other alias. In state *C C* both aliases are rewarded by both actors. In state *D D* neither alias is rewarded by either actor. In state *C D* both aliases are rewarded by actor 1 only. In state *D C* both aliases are rewarded by actor 2 only. In state *A A* each alias is rewarded by his own actor. In state *O O* each alias is rewarded by the other actor.

These states will be called *balanced*. In all the other ten states the total reward received by one alias is different from the total reward received by the other alias. These states will be called *unbalanced*.

In conclusion the intrapersonal subsystem is balanced when both its states are either homovalent or both are heterovalent *and* equal.

The interpersonal subsystem.

We turn now to the interpersonal subsystem for a given actor. This is the subsystem formed by the states of both observers for a given actor. The 16 possible combinations of states of this subsystem are given in Table 3. Let us consider the state of observer 1. We know already that this state indicated whether this observer does

TABLE 3
THE INTERPERSONAL SUBSYSTEM FOR ACTOR 1

State and Class				Valence Pattern			
of observer 1		of observer 2		of observer 1		of observer 2	
state (1)	class (2)	state (3)	class (4)	for alias 1 (5)	for alias 2 (6)	for alias 1 (7)	for alias 2 (8)
C	*B*	*C*	*B*	+	+	+	+
D	*B*	*D*	*B*	−	−	−	−
C	*P*	*D*	*P*	+	+	−	−
D	*P*	*C*	*P*	−	−	+	+
A	*E*	*A*	*E*	+	−	+	−
O	*E*	*O*	*E*	−	+	−	+
A	*R*	*O*	*R*	+	−	−	+
O	*R*	*A*	*R*	−	+	+	−
C	*B*	*O*	*R*	+	+	−	+
D	*B*	*A*	*R*	−	−	+	−
O	*E*	*C*	*P*	−	+	+	+
A	*E*	*D*	*P*	+	−	−	−
O	*R*	*D*	*B*	−	+	−	−
A	*R*	*C*	*B*	+	−	+	+
D	*P*	*O*	*E*	−	−	−	+
C	*P*	*A*	*E*	+	+	+	−

or does not perceive a conflict between rewarding himself (alias 1) and rewarding the other (alias 2). In order to know whether the conflict really exists one needs to compare the valance of observer 1 for alias 1 with the valance of observer 2 for alias 2. When these two valences are different, then the conflict really exists. Otherwise it may be merely perceived by observer 1. Indeed the valence of observer 1 for alias 2 is nothing but the image that this observer has of the reward of the other.

This comparison between the existence of the conflict and its perception by a given observer suggests four classes of states for this observer:

1. The conflict exists and is not perceived. This is the class of projection or *P* class: the observer projects on the alias of the other his own valence and fails to judge correctly the valence of the other. For example, in state *C D* of Table 3, the valence that observer 1 attributes to the alias of 2 (column *6*) is positive and identical with the valence that this same observer attributes to his own alias (column *5*), but different from the valence that observer 2 attributes to his own alias (column *8*). In consequence the valence of column 5 is different from the valence of column *8*, i.e., the conflict really exists. Likewise the valence that observer 2 attributes to the alias of 1 (column *7*) is equal to the valence that the same observer attributes to his own alias (column *8*). Thus in state *C D* both observers are in the *P* class, i.e., both observers fail to perceive the existing conflict.

2. The conflict exists and is perceived. The observer judges correctly the valence of the other which is different from his own. This is the *empathic* or *E* class. In state *A A*, for example, both observers are in the empathic class.

3. The conflict neither exists nor is perceived. The valence attributed to the alias of the other is equal to both the valence of the alias of the self and the valence attributed by other observer to his own alias. Here one has *both* empathy and projection, or the *B* class. This happens, e.g., in state *C C* for both observers.

4. The conflict does not exist but is perceived. The valence attributed to the other is different from both the valence of the self and the valence of the other. The last two valences are, of course, equal. This is the class of *rejection* or *R* class.

When there is no real conflict the valences of the two observers for their respective aliases (columns *5* and *8*) are identical and each observer can be in either class *B* or class *R*. This will be called *similarity condition*. When there is conflict the valences of columns *5* and *8* are different and each observer is either in class *P* or in class *E*. This will be called *dissimilarity condition*. It follows that when one observer is either in class *P* or class *E*, the other observer must also be in one of these two classes. When one observer is in class *B* or in class *R*, the other observer must also be in one of these two classes. Similarity or dissimilarity, i.e., the absence or presence of real conflict in the distribution of rewards, applies equally well to both observers.

The class of one observer does not depend on the specific state of this observer alone, but also on the state of the other observer. In particular the following necessary relationships between states and classes exist:

1. When the states of both observers are homovalent and identical (*C C* or *D D*), both observers are in the projective-empathic class.

2. When the states of the two observers are homovalent but different (*C D* or *D C*), both observers are in the projective class.

3. When the states of the two observers are heterovalent and alike (*A A* or *O O*), both observers are in the empathic class.

4. When the states of the two observers are heterovalent and different (*A O* or *O A*), both observers are in the rejective class.

An interpersonal subsystem is *balanced* when the states of both observers are either homovalent or heterovalent. Otherwise the subsystem is *unbalanced*. The eight states mentioned above appear in the upper half of Table 2, and are all balanced. In these states both observers belong to the same class. Therefore:

Theorem: When the interpersonal subsystem is balanced, the two observers belong to the same class. When the subsystem is

unbalanced, the two observers belong to a different class. In unbalanced subsystems, when one observer is in the empathic class the other must be in the projective class. When one observer is in the rejective class, the other must be in the empathic-projective class.

Let us disregard the fact that empathy may or may not be accompanied by projection, depending on the presence or absence of a real conflict in the distribution of reward. Then the states of the two observers, for a given actor, are balanced when either both observers or neither are empathizing. In the first case, the balance is achieved, so to speak, through good communication on both sides. In the second case, it is precisely the lack of communication that produces balance: each observer perceives his own social "reality" with little regard for the perception of the other.

On the other hand, when only one observer empathizes neither isolation nor full mutual understanding is possible: the symmetric balance is broken. If one of the two observers is a better empathizer than the other (for example, he might be the leader of the dyad), one cannot expect balance in this particular subsystem. However, it seems possible that the leader might use his superior judgment of the interpersonal situation in order to manipulate it, i.e., strive toward a given balanced state.

The psychological meaning of balance.

An *intrapersonal* subsystem has been defined as balanced when both its states are either homovalent or heterovalent and equal. An *interpersonal* subsystem has been said to be balanced when both its states are either homovalent or heterovalent.

The dyadic system will be said to be balanced when all its subsystems are balanced. We may also agree that a state will be called balanced when its two valences are equal, i.e., when the state is homovalent.

All these definitions of balance rest on formal properties of the system. Yet they reveal clear-cut psychological differences between balance and imbalance. These differences can be summarized as follows:

1. In the state for a given observer and actor, balance or homovalence, indicated lack of conflict between self reward for the acting person and reward for the other person.

2. In the intrapersonal subsystem balance refers to the distribution of rewards between the two aliases. When the subsystem is balanced both aliases receive the same amount of reward. When the subsystem is unbalanced one alias is rewarded more than the other.

3. In the interpersonal subsystem balance refers to the communication between the two observers. When the subsystem is balanced the communication from observer 1 to observer 2 is equally good as the communication from observer 2 to observer 1. Either each observer judges correctly the perception of the other, or both misjudge it. Imbalance indicates that while one observer empathizes the other, the other does not empathize him.

In the whole system balance refers both to distribution of rewards and to communication. When the whole system is balanced there exists a necessary relationship between the classes of the two observers relative to both actors and their respective perception of the distribution of rewards. In particular:

1. When both observers are in the empathic (or empathic-projective) class, with respect to both actors, their respective perceptions of the distribution of rewards are balanced and alike.

2. When neither observer is in the empathic (or empathic-projective) class, with respect to both actors, the respective perceptions of the distributions of rewards are balanced but different.

Mutual empathy is therefore a necessary and sufficient condition for the two observers to have the same perception of reward distribution. Lack of empathy, on both sides, is a necessary and sufficient condition for the two observers to have different perceptions of the reward distribution.

The formal concept of balance used in this paper is somewhat more restricted than the concept developed by Cartwright and Harary (1), following certain hypotheses of Heider (9) and Newcomb (11). The reason for adopting here more restricted definition

of balance may be found in the fact that our system extends over three dimensions: actor, observer, and alias. Each subsystem covers two of these dimensions. A given valence in a certain subspace does not necessarily balance an equal valence in another subspace.

HYPOTHESES

There are four possible states for each observer and for a given actor. This makes 16 states for each observer and both actors, and $16 \times 16 = 256$ states for the dyad as a whole. The purpose of empirical hypotheses is to predict which states are more likely to appear in practice and which are less likely. To put it differently, we are concerned here with the possible relationships between the state of one observer for a given actor and that of the same observer for the other actor; between the state of one observer for a given actor and that of the other observer for the same actor, etc. It is evident that, if these states are interdependent, the frequencies of the 256 possible states will vary and some states may not appear at all.

The general balance hypothesis.

Newcomb (11) and Heider (9) have suggested that systems tend toward a balanced state. This might be interpreted to indicate that, in a given population of systems, it can be expected that the frequency of the balanced states will be higher than that of the unbalanced states. This general hypothesis, in terms of the present model, suggests the following two specific hypotheses:

1. A person tends to perceive that he and the other get the same amount of reward from their relationship (balanced intrapersonal subsystem).

2. A person tends to judge correctly the rewards perceived by the other to the extent that the other succeeds in judging correctly the rewards perceived by him (balanced interpersonal subsystem).

If these predictions should prove true, it will be necessary to explain the transition from balanced to unbalanced states, and vice versa. One possible explanation is that the system may move from an unbalanced state to a balanced state (or to another unbalanced state), but not vice versa: Once the system is balanced it does not return to an unbalanced state. Thus the unbalanced state would be identical with the transient state of the mathematicians (3, p. 323). The balanced state would operate something like a trap: one can get into it, but he cannot get back to the unbalanced state again. If this explanation is accepted, one would predict that the frequency of balanced states will increase with the age of the system: the longer the system has been in existence, the higher the frequency of balanced states.

An alternative explanation is as follows: unbalanced states are states of passage from one balanced state to another. The system, in order to move from one balanced state to another, must pass through an unbalanced state. Moving to an unbalanced state is somewhat like getting into an automobile in order to reach a different place. Some formal support for this hypothesis is found in the valence pattern of Table 3. Consider the pattern of any balanced state; it is evident that if one, and only one, of the valences changes, the pattern becomes unbalanced. Vice versa, any single change of valence in an unbalanced pattern makes it balanced. According to this explanation, therefore, going into an unbalanced state is a necessary intermediate step toward a different balanced state. The identification of imbalance with state of change suggests that interpersonal subsystems are more likely to be unbalanced under conditions of dissimilarity than under conditions of similarity; imbalance seems more likely when there is a real conflict between the rewards that the two observers attribute to their respective aliases.

Thus one might formulate the following:

Hypothesis: In interpersonal subsystems, states of balance are associated with similarity, and states of unbalance with dissimilarity.

Under conditions of dissimilarity the possible classes of the two observers are *P* and *E*, the projective and the empathic. The state is unbalanced when the class combinations of the two observers are *P E* or *E P*. If the two observers are in the same class

(*P P* or *E E*), the state is balanced. Now, under conditions of dissimilarity and unbalance, which observer will more likely be in the *E* class and which one in the *P* class? It is proposed that the one of the two observers, who is also the actor, is more likely to be in the *E* class than the other observer. Being in this class enables the actor-observer to ascribe correctly the valence of the other, which is a prerequisite of any attempt to change this valence. One possible way of changing it is through behavior. Thus the actor, in order to decide which behavior is required to change the valence of the other, must be in the empathic class, i.e., he must know the present valence of the other. Therefore, it seems possible to suggest the following:

Hypothesis: In interpersonal subsystems, the frequency of the class combination *E P* is higher than that of the combination *P E* (as usual, the first letter indicates the class of the actor, and the second that of the non-actor).

This hypothesis seems to contrast with earlier findings showing the ego-involvement hampers empathy (7, 10). It should be noted, however, that empathy as discussed here refers neither to perception of behavior nor to the norm, but to the *relationship* between them. Although ego-involvement may produce misjudgment of the behavioral perception of the other and/or of his norm, it does not necessarily follow that it should also cause misjudgment of the valence between perception of behavior and the norm. Mistakes in judging perception and the norm might cancel out each other and result in a correct judgment of the relationship between them.

The dissonance hypothesis.

Festinger (4) has suggested that dissonance is associated with pressure to reduce or eliminate such dissonance. In our system dissonance between perception of action and norm is identical with lack of reward. Thus Festinger's hypothesis would indicate that a behavior perceived as producing an increase of reward is more likely to occur than a behavior producing a perceived decrease.

On the other hand, it might be supposed that a relationship is more likely to continue the larger the amount of reward it produces. The relationship will tend to cease when the system is characterized by a large number of dissonances, i.e. by little or no reward. In summary: either the dissonance will change to consonance or the relationship will tend to terminate. In both cases it may be expected that:

Hypothesis: In a population of dyads the frequency of the state of each subsystem is inversely related to the number of its dissonances.

This hypothesis is in agreement with the principle of modern behavior theory that a given behavior tends to cease unless it is rewarded.

The actor hypothesis.

It is suggested that, in heterovalent states, consonance will tend to be associated with the observer's own behavior more than with the behavior of the other. This is more or less equivalent to saying that one is more likely to follow the behavior that gives reward to him than the behavior that rewards the other.

PROCEDURE

A group of 361 factory workers and their respective 51 foremen were administered the Test of the Foreman-Worker Relationship (6). This test consists of two sets of picture stories, with eight stories in each set. One set refers to the behavior of the foreman, and the other set to that of the worker. Foremen and workers alike were requested to complete each story by choosing an appropriate picture from four alternatives. Respondents were requested to indicate both the picture which depicts what usually happens in practice in their department, and the picture which shows what ought to happen in ideal conditions. Thus for each respondent four sets of eight responses each were obtained, referring respectively to the actual behavior of the worker, the actual behavior of the foreman, the ideal behavior of the worker, and the ideal behavior of the foreman.

Furthermore, each worker was asked to

guess the responses of his foreman, while the foreman was asked to guess the modal responses of his workers.

The valence of a given respondent for each story was determined by observing whether the response at the actual level was the same as or different from the corresponding response at the normative level. Thus eight valences were obtained for each set of eight stories. Obviously there were three possibilities: (a) the frequency of consonances was higher than that of dissonances; (b) the frequency of dissonances was higher than that of consonances; (c) the two frequencies were equal (four consonances and four dissonances).

In the first two cases the valence with the modal frequency was accepted as the prevailing valence of the respondent. In the third case the prevailing valence was said to be uncertain. Further, for the respondents with a prevailing valence, the state of each respondent was determined by observing the prevailing valence of the responses he ascribed to himself and the prevailing valence of those he ascribed to the other. This determined the prevailing state of the respondent for the behavior of the actor to whom the set of stories referred. The respondents with at least one uncertain valence were said to be in an uncertain state.

This procedure established the prevailing or modal state for each respondent. It would have been possible, instead, to find the frequency distribution of the various states for each respondent. It was felt however, that because of the preliminary nature of the present analysis and in view of the relatively small number of cases, a full analysis of the frequency distribution would have been premature. Respondents of uncertain state are not included in the present analysis.

RESULTS[2]

In presenting the results, the states of the *worker*, relative to both his own behavior and that of the foreman will be referred to as *subsystem 1* or the intrapersonal subsystem of the worker. The states of the foreman relative to both his own behavior and that of the worker will be referred to as *subsystem 2*, or the intrapersonal subsystem of the foreman.

The states of both the worker and the foreman relative to the behavior of the *worker* will be referred to as *subsystem a*, or the interpersonal subsystem for the worker's behavior. The states of both the worker and the foreman relative to the behavior of the *foreman* will be referred to as *subsystem b*, or the interpersonal subsystem relative to the behavior of the foreman.

The general balance hypothesis

In each one of the four subsystems the frequency of the balanced states is always higher than that of the unbalanced states. In subsystem b, however, the two frequencies are almost identical (see Table 4).

The breakdown by "age" of the dyad shows that:

1. Dyads in the first year of existence have a relatively high intrapersonal balance (subsystems 1 and 2) and a relatively low interpersonal balance (subsystems a and b).

2. Dyads in the second and third years of existence have a higher interpersonal balance and a lower intrapersonal balance.

3. Dyads older than three years show a certain downward trend in balance in all four subsystems.

The small number of cases in each cell makes these results highly tentative. They seem to indicate, however, that balance does not tend to increase with time, and there-

TABLE 4

PERCENTAGE OF BALANCED STATES BY "AGE" OF DYAD

Subsystem	Percentage of Balanced States for Various "Ages" of Dyad				
	less than one year	1–1½ years	1½–3 years	more than 3 years	all ages
	%	%	%	%	%
1	78	52	75	66	72
2	100	100	99	66	82
a	63	75	73	64	70
b	33	55	54	50	51

[2] All the differences reported as significant are significant at the 1% level at least. Actually almost all the differences are significant at the 0.1% level. The chi-square test was used.

TABLE 5

PERCENTAGE OF BALANCER STATES FOR DIFFERENT CONDITIONS OF SIMILARITY

Condition	Percentage Frequency of Balanced States	
	Subsystem a	Subsystem b
Similarity	76	81
Dissimilarity	52	32

fore do not support the hypothesis that unbalanced states are transient states.

Relationship between balance and similarity

In the two interpersonal subsystems, a and b, the frequency of the balanced states is significantly higher under conditions of similarity than under conditions of dissimilarity (see Table 5).

These results sustain the hypothesis relative to the existence of a positive relationship between balance and similarity. Conditions of similarity are more frequent for the behavior of the worker (75% of the cases) than for that of the foreman (37% of the cases).

Relative frequency of the E P and P E classes

In subsystem a the frequency of the *E P* class is almost four times that of the *P E* class. In subsystem b the frequency of the *E P* class is more than twice that of the *P E* class. These significant differences seem to sustain the hypothesis that, under conditions of dissimilarity and imbalance, the actor is more likely to be in the *E* class than is the non-actor.

The dissonance hypothesis

Table 6 shows that in all of the subsystems except for subsystem b, the frequency of each state is inversely related to the number of its dissonances. This relationship is particularly clear-cut in subsystems 1 and a. In subsystem b the relationship does not appear. A possible explanation is suggested by the intermediate position of the foreman in the social structure of the factory. The behavior of the foreman is influenced not only by the needs of his relationship with the worker but also by his contact with management.

TABLE 6

PERCENTAGE FREQUENCY OF STATES WITH A GIVEN NUMBER OF DISSONANCES

Number of Dissonances	Frequency in Subsystem			
	1	2	a	b
0	54	82	53	25
1	20	18	25	43
2	14	—	15	25
3	7	—	4	7
4	5	—	3	—
All states	100	100	100	100
Absolute frequency	151	23	159	159

Note. The difference between the absolute frequencies given above and the total number of workers and foremen gives the number of cases, in each subsystem, for which there was no prevailing state for at least one of the two states of the subsystem.

In general the results reported in Table 5 seem to sustain the hypothesis that dissonance either changes to consonance or tends to produce a discontinuance of the relationship. In other terms: the likelihood of a dyad to continue is positively related to the amount of reward it produces.

Actor hypothesis

According to this hypothesis the frequency of state *A* should be higher than that of state *O*, and especially so for the foreman's behavior. The data do not sustain this hypothesis. They suggest instead that the relative frequency of states *A* and *O* is related to the observer rather than to the actor. In the worker the frequency of state *A* (for the behavior of the worker and the foreman combined) is two-and-a-half times the frequency of state *O*. In the foreman on the contrary, the frequency of states *O* is doubled that of state *A*. These differences are highly significant. Thus the manner of distribution of the perceived rewards seems to be a characteristic of the observer rather than of the actor.

The main findings may be summarized as follows:

1. Both the intrapersonal and the interpersonal subsystems tend to be balanced. That is (a) Each member of the dyad tends to perceive that he and the other get the same amount of reward from the relation-

ship; and (b) Each member of the dyad tends to judge correctly the rewards perceived by the other to the extent that the other succeeds in judging correctly the rewards perceived by him.

2. The frequency of the balanced states does not increase with the age of the relationship. This finding supports the hypothesis that imbalance is a state of transition from one balanced state to another.

3. In the interpersonal subsystem balance is positively related to similarity; imbalance to dissimilarity. This finding suggests that the interpersonal subsystem is more likely to be unbalanced, or in the process of changing, when the reward perceived by one observer is different from the reward perceived by the other observer with regard to the same action.

4. Under conditions of dissimilarity and imbalance the actor is more likely to be in the empathic class than the non-actor. That is: each one of the two persons of the dyad is more likely to judge correctly the reward received by the other when the reward is produced by the behavior of this same person than when it is produced by the behavior of the other person.

5. The frequency of a given state of a subsystem tends to be inversely related to the number of its dissonances. This finding suggests that a dyadic relationship is more likely to continue the larger the amount of reward it produces.

6. The relative frequency of states *A* and *O* is related to the observer rather than to the actor. This finding suggests that the way of solving the conflict between rewarding the self and rewarding of the other is related to the perception of behavior rather than to behavior in itself.

DISCUSSION: TOWARD A STOCHASTIC THEORY

The empirical results seem to suggest the existence of three main tendencies in the model:

1. A tendency toward balancing rewards both intra- and interpersonally.

2. A tendency of the actor to choose the behavior that will improve communication of reward, that is the behavior that will be perceived by the other in the same way as it is perceived by the actor.

3. A tendency to choose the behavior which is perceived as associated with reward rather than the behavior perceived as non associated with reward.

If the choice of future behavior will be determined by these tendencies, it seems that findings are suggestive of new hypothesis concerning the probabilities of transition from one given state to another. This may provide a contribution toward a stochastic theory of interpersonal behavior.

In order to formulate these new hypotheses, let us assume that the operation of moving from one state to another consists in changing the sign of one of the valences of the state, and of one valence only.

It then follows immediately that:

1. There is zero probability of moving from a balanced state to another balanced state, and from an unbalanced state to another unbalanced state.

The findings on the frequencies of balanced and unbalanced states suggest that:

2. The probability of being in a balanced state is higher than that of being in an unbalanced state.

The findings relative to the relationship between balance and similarity suggest that:

3. Under conditions of similarity an unbalanced state has a relatively high probability of moving to a balanced state.

4. Under conditions of dissimilarity a balanced state has a relatively high probability of moving to an unbalanced state.

The findings relative to the relationship between the number of dissonances of the state and its frequency suggest that:

5. A valence change from dissonance to consonance is more probable than one from consonance to dissonance.

The findings relative to the relationship between the *E* class and the actor suggest that:

6. In a move from a balanced to an unbalanced state a change in the valence of the actor is more probable than a change in the valence of the nonactor. Further: a change in the valence attributed to the

other (the non-observer) is more probable than a change in the valence attributed to the self (the observer).

7. In a move from an unbalanced to a balanced state a change in the valence of the non-actor is more probable than a change in the valence of the actor. Further: a change in the valence attributed to the self is more probable than a change in the valence attributed to the other.

While the above hypotheses, as they stand, lend themselves to experimental verification, it seems that further conceptual work is required in order to integrate them into a stochastic theory of interpersonal behavior.

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(Manuscript received March 7, 1958.)

. . . There are at least six personalities distinctly to be recognized as taking part in [the] dialogue between John and Thomas.

Three Johns:
1. The real John; known only to his Maker.
2. John's ideal John; never the real one, and often very unlike him.
3. Thomas's ideal John; never the real John, nor John's John, but often very unlike either.

Three Thomases:
1. The real Thomas.
2. Thomas's ideal Thomas.
3. John's ideal Thomas.

Only one of the three Johns is taxed; only one can be weighed on a platform-balance; but the other two are just as important in the conversation. . . . No wonder two disputants often get angry, when there are six of them talking and listening all at the same time.

OLIVER WENDELL HOLMES in *The Autocrat of the Breakfast-Table*

This article discusses the concept of "normality," or emotional health, and describes various methods by which it may be evaluated.

CONCEPTS OF NORMALITY OR EVALUATION OF EMOTIONAL HEALTH

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THOUGH much has been written and is known about endopsychic and environmental factors which predispose to emotional ill health, comparatively little research has been carried out regarding the etiology of emotional health. I am a member of a team[1] composed of psychiatrists, sociologists, and a psychologist that has been engaged for some time in a study of emotionally healthy adolescents and their families with the aim of attacking this crucial problem and of establishing bridging concepts between the disciplines involved. The specific objective of this paper is to convey observations and conclusions regarding the concept of normality which had to be clarified as a necessary baseline for the study.

Clarification of Terms. The word "normality" as used in mental health is an overworked and ambiguous term. The dictionary definition of the adjective "normal" is "according to an established standard, rule or principle, conforming to a norm; regular; natural." Generally a person is regarded as normal if what he says and does conforms with certain standards laid down by the culture and social group to which he belongs. Eccentricity of behavior and even originality of thought are often regarded as abnormal. Behavior acceptable to one status group, age group, or sex would not be acceptable to another. Normality in this sense signifies that an individual fulfills the social role or roles expected of him. Subjective views of normality may coincide neither with objective views nor with emotional health. For instance, many paranoid schizophrenics may regard themselves as normal and emotionally healthy although in the view of others they are not.

In our project we have come to use the term "emotionally healthy" instead of "normal." We feel it is a more positive term, but it is also difficult to define. As Erikson (3) states, it is difficult to say what is health as contrasted to what is not healthy or what is not unhealthy. Also it is much easier to describe what a healthy person ought to be like than it is to describe what he is or can be. Health implies a state of surplus energy, vitality, and awareness.

Quantitative Approach

The first and perhaps most obvious approach to the problem of evaluation of emotional health is through the use of the statistical method.

As a preliminary step it appears essential to differentiate the statistical term "average" from such a term as "normal" or "emotionally healthy." According to King (9) "average is that which results from mathematical computation based upon more than one sample; i.e., a specimen mean . . . normal that which functions according to its inherent design, i.e., pattern normal." In psychiatry, particularly in the field of personality assessment, that which is statistically average is certainly not always normal or healthy. Kinsey's studies (10) might be an example of this point. His oft-disputed figures on orgasm in the female seem to show that a high percentage of women are rarely orgastic in intercourse. This is quite different

[1] The other members of the research team are Drs. Eric Wittkower, Wm. Westley, Fred Elkin, and Nathan Wisebord. The author wishes to express his gratitude to them for their valuable stimulation and contribution during this work.

from what psychiatrists would feel should be considered healthy functioning. His figures indicate that a high percentage of women do not function according to their biological inherent design.

The statistical method has many operational limitations when dealing with data available to us so far in the field of psychiatry. It would seem that the effectiveness of this method is in inverse proportion to the number of uncontrollable variables involved in what is being measured.

The further difficulties in utilizing the statistical approach are partly due to our lack of relevant data in many areas of personality functioning. We are not often sure what should be measured, and furthermore in many instances we are unclear as to what relationship that which is being measured has to other variables. Basically a the variables involved in personality functioning have not yet been clearly isolated and delineated.

This does not mean that we argue against attempts to describe behavior in quantitative terms. All we intend to do is to point out the limitation of a statistical approach to these problems at the present state of our knowledge. It is hoped that this effort will clarify some areas in future attempts to evolve a more satisfactory statistical approach.

Regardless of how refined a statistical method is eventually developed, it is likely that the following statements of King (9) would still apply: mathematical computation can never take the place of empirical ascertainment regarding an organism functioning in accordance with design. What is in fact healthy can never be ascertained simply by use of a mathematical tool because its essential dependence is upon *qualitative consideration* and mathematics deals with *quantitative data.*

The concept of motivation in behavior is important in this context. It is extremely difficult to quantify motivation. Much of its evaluation depends on qualitative analysis. The same behavioral sequence in either the same or different individuals may be healthy or unhealthy depending upon the motivational dynamics involved.

This fact is demonstrated by the following cases:

Case 1: Subject is an 18-year-old Physical Education student. She is tall and rangy, quite masculine in appearance, carriage, and manner of speech. She is very tense and has considerable difficulty in relating to the interviewer. She displays marked emotional constriction and blocks frequently throughout the interview.

Her consuming interest in life is athletics, and she is extremely proficient in all types of sports. She has been interested in Physical Education ever since her 9th year at school.

Her background history reveals considerable psychopathology. She was a marked feeding problem in infancy and childhood. She states "mother had to push food down me, I was sickly during the first year of life. I don't know what was the matter, but I was sick at the stomach all the time and no one ever thought I would live".

She has one brother who is 4 years older. She is very vague in her description of him, except to say that they fought all the time. When asked as to who used to win these fights she stated "he was bigger."

She describes the family structure as one where there was almost a complete dichotomy between the women and the men in the family, with very little communication between the two pairs. She speaks of her mother with gentle contempt, indicating mother may be a nice person but unfortunately she is too weak. She expresses resentment of the mother for having been so lenient with her.

The father is described in somewhat similar terms; i.e., he is kind and generous, but she is covertly contemptuous of him because he allows her to dominate him. She feels close to mother but not to father.

Father was an excellent athlete in his youth and strongly encouraged both children in the area of athletics. It seems that the subject's life has been dominated by a strong desire to outdo her brother in athletic powers. This was difficult for her at first, because he was a boy, four years older and had considerable athletic ability as well. However, she finally succeeded in surpassing him as an athlete because of her ability and intense concentration on this area, plus the fact that he wasn't that interested.

Subject has no interest whatsoever in dating boys. She would like to teach physical education and biology on the high school level, but her real ambition is to specialize in basketball at the college level.

She does not want to get married because she feels she is domineering and would like to be her own boss and since most men are domineering she wants to avoid a conflict in this sphere. In almost the same breath she states however that she has always had a strong desire to breed horses because of her love for them.

Case 2: Subject is a 16-year-old Physical Education student. She is neatly dressed in an attractive feminine manner. At the beginning of the interview she is somewhat reserved and distant, but this plus her initial tension lessens as the interview progresses.

She has always liked athletics and has thought of studying Physical Education since public school days. However when first seen she was beginning to doubt whether she wanted to continue in this field.

She describes her parents in warm and positive terms. The father is described as a friendly, gregarious person. Mother is described as quieter, more intellectually oriented, but as being the firmer of the two parents. She feels close to and comfortable with the mother.

Subject has mild conflicts in the areas of expression of rage and in close intimate relationships with her peers. She goes on frequent dates and has been going steady for several months. She wants to teach for a few years after college, then wants to marry and raise a family.

The childhood and developmental history is notable only for the presence of eczema in the limb flexures and between the fingers since the age of 3 months. There has been concomitant seasonal hay fever since the age of 5. At the present time there is only a minimal amount of eczema between the fingers of one hand. Her brother had eczema until age 14, and her father has hay fever. As a result of the eczema the subject's arms were tied and bandaged at night until 3 years ago.

This seems to be a most important fact in this case in the light of findings reported by Mittelman (14).

The motivation for this subject's interest in athletics would seem to have developed largely as a result of the restriction of movement necessitated by the handling of her eczema. This is in contrast to what is seen in Case 1, where the motivation for an athletic career probably developed primarily as a result of marked confusion of sexual identity and conflicts over aggression.

When last seen, Case 2 had definitely changed her university course to Physiotherapy and was a leading member of a semiprofessional group of drum majorettes.

Qualitative Approach

The predominant psychiatric approach to the concept of normality has been the clinical or adaptive approach. Psychiatrists well grounded in psychodynamics and psychoanalysts have paid particular attention to the integrative aspects of the individual's dynamic adaptation. As Redlich (16) points out, most statements on the subject are normative propositions which implicitly express value judgments.

The propositions so far put forward vary from broad generalized statements to more specific attempts at sharper definition of the problems involved.

An interesting example of one of these generalized statements is one which Erikson (3) quotes as attributed to Freud. "Freud was once asked what he thought a normal person should do well. Freud is reported to have said, "Lieben und Arbeiten" (to love and to work). On first glance this statement appears to be a superficial banality. However, the deeper one goes into it the more apparent the truth and wisdom in this cryptic answer becomes. The effectiveness of this statement lies in its ability to be applied on a transcultural basis. In cultures such as ours we see men able to work quite effectively but with minimal abilities to love in all aspects of that concept. This order is reversed in other cultures. In some cultures such as the Alor they had great difficulties in both (8).

Other generalized normative propositions are those by Karl Menninger and Erich Fromm.

Menninger (13) states, "Let us define mental health as the adjustment of human beings to the world and to each other with a maximum of effectiveness and happiness. Not just efficiency, or just contentment—or the grace of obeying the rules of the game cheerfully. It is all of these together. It is the ability to maintain an even temper, an alert intelligence, socially considerate behavior, and a happy disposition. This, I think, is a healthy mind."

Fromm (4) says, "The term normal or healthy can be defined in two ways. Firstly, from the standpoint of functioning society one can call a person normal or healthy if he is able to fulfil the social role he is to take in that given society,—if he is able to participate in the reproduction of society. Secondly, from the standpoint of the individual, we look upon health or normalcy as the optimum of growth and happiness of the individual."

The Expert Committee on Mental Health

of the World Health Organization has defined mental health as the "capacity to establish harmonious interpersonal relationships."

Heath, *et al.* (6), in their introductory report on the Grant Study, define normal as "the balanced, harmonious blending of functions that produce good integration. . . . The "normal" individual, therefore, here is regarded as the *balanced* person whose combination of traits of all sorts allows him to function effectively in a variety of ways."

Kubie has long been working on this problem. In an interesting article (11) he attempts a very specific answer to the question. His very approach seems to be a tacit admission of the complexity of the problem. He does not try to define directly the distinction between a normal and neurotic personality but merely focuses on behavioral acts and draws implications therefrom.

His basic thesis appears to be that a behavioral act is neurotic "whenever the Unconscious system (or perhaps an alliance between the Preconscious and Unconscious system) predominates" (in the production of that behavior). Such acts "will not be responsive to the experience of pleasure, of pain, or to rewards and punishments, or to logical argument—neither to the logic of events, nor to any appeals of mind or heart."

On the other hand, normal behavior results "whenever an alliance of the Conscious and Unconscious systems predominates" in the production of that behavior. "Such behavior never becomes either insatiable or stereotyped. It can be altered by the experience of success and failure, of rewards and punishment, of pleasure and pain. . . . It is therefore anchored in reality, yet it remains freely flexible."

Kubie's descriptive passages appear to be brilliantly flawless. However, when it comes to defending his explanation of the genetic locus of such behavior he resorts to logical processes which appear to be questionable. When his statements are challenged by the observation that not all hidden depths of the personality are malevolent, but may even be creative, he responds by saying he agreed with these statements but that this sort of nonpathogenic behavior originates predominantly in the Preconscious and not in the Unconscious system. Thus he feels his hypothesis is still valid.

Rado (15) avoids the necessity for such debatable reasoning by stating that in maladaptive behavior "the individual's behavior is controlled, without his being aware of it, by emotional patterns which overrule his reason."

Hartmann (5) also expresses this same concept effectively, although using a different approach. He mentions the apparent paradox of two long held and apparently opposing psychoanalytic conceptions of normality, i.e., instinctual freedom and rational behavior. He disposes of this apparent conflict by stating that "the rational must incorporate the irrational as an element in its design." He then goes on to speak of the concept of "freedom." On one level it is "control exercised by means of the conscious and preconscious ego." On another level: "A healthy ego must evidently be in a position to allow some of its most essential functions, including its 'freedom', to be put out of action occasionally, so that it may abandon itself to 'compulsion' (central control). This brings us to the problem of a biological hierarchy of the ego's functions and to the action of the integration of opposites, which we have already met in connection with the problem of rational conduct."

Hartmann's ideas of a biological hierarchy of ego functions are strikingly similar to Maslow's (12) concept that: "Basic human needs are organised into a hierarchy of relative prepotency. One main implication of this phrasing is that gratification becomes as important a concept as deprivation in motivation theory, for it releases the organism from the domination of a relatively more physiological need, permitting thereby the emergence of other more social goals. The physiological needs, along with their partial goals, when chronically gratified cease to exist as active determinants or organizers of behavior. They now exist only in a potential fashion in the sense that they may emerge again to dominate the organisms if they are thwarted. This statement is

qualified by a hypothesis that it is precisely those individuals in whom a certain need has always been satisfied who are best equipped to tolerate deprivation of that need in the future, and that furthermore, those who have been deprived in the past will react differently to current satisfactions than the one who has never been deprived."

In Maslow's book (12) he reports on a fascinating study he conducted on what he calls "self-actualizing" people. This latter term appears to be the equivalent of "emotionally healthy," as we use that term in our study. Maslow's ideas are similar to ours, as for example, when he states: "full health and normal and desirable development consist in actualizing man's inner inherent nature and potentials."

The writings in the field of classical psychoanalysis have very little to offer in this field from a positive standpoint. Maslow comments on this same point when he states "The science of psychology has been far more successful on a negative than on the positive side; it has revealed to us much about man's shortcomings, his illnesses, his sins, but little about his potentialities, his virtue, his achievable aspirations, or his full psychological height. It is as if psychology had voluntarily restricted itself to only half its rightful jurisdiction, and that the darker, meaner half. This is no extrinsic, superficial attitude; it seems clear rather that it is intrinsic and at the heart of the whole culture."

Maslow's observations that various psychoanalytic writers describe ideal love relationships so as to make them into a task or burden, rather than a game or a pleasure is very clearly illustrated in an interesting and well-written article by Balint (2) "On Genital Love." To the psychoanalyst, at least in the past, the individual reaching the "genital level" of development, as defined in the classical literature, was a mature, emotionally healthy individual. Some excerpts from Balint's paper are hereby offered in order to illustrate the approach by classical psychoanalysis to the problem:

"What we call "genital love" is an artefact of civilization like art or religion. It is enforced upon us, irrespective of our biological nature and needs, by the condition that mankind must live in socially organized groups. . . .

The demand for prolonged, perpetual regard and gratitude forces us to regress to, or even never to egress from, the archaic infantile form of tender love. Man can be regarded therefore as an animal which is retarded even in his "mature" age in an infantile form of love. . . .

This work (of developing and maintaining a genital love relationship) causes a considerable strain on the mental apparatus and only a healthy ego is able to bear it."

We believe that the findings of Balint's article and similar writings in this field have been colored by two factors of which their authors did not take account. These factors are (1) Their observations, although valid and well reported, were made on sick people. Thus they were reporting on sick people's reactions to mature pleasures, tasks, and opportunities. Instead of being reacted to as pleasant and fulfilling, and grasped at eagerly, they were indeed responded to as frightening, burdensome tasks. The mistake these writers made was to describe mature behavior in these terms.

(2) Many of the concepts underlying their observations and interpretations were themselves based upon unrealistic and/or disproved premises. These include: (*a*) invalid and oft disproven cultural anthropological theories; (*b*) false analogies drawn from the fields of animal psychology, embryology, and anatomy, (*c*) Calvinistic and Puritanical philosophical theses underlying Western culture. These implied that man is made up of evil, dark, animal forces, which must constantly be held in check in order for him to be able to live in a "civilized" society.

Contrast Balint's views with a statement from Maslow (12) based upon his studies of healthy individuals:

> Loving at a higher need level makes the lower needs and their frustrations and satisfactions less important, less central, more easily neglected. But it also makes them more wholeheartedly enjoyed when gratified.

Cultural values and beliefs are thus of major importance in determining the theo-

retical concepts held regarding emotional health, as shown by these excerpts from the literature. Furthermore, they appear to be of paramount importance in the process of the development or hindrance of positive emotional health.

As a result of our preliminary findings in this project we agree with Ackerman (1) when he says that mental health in positive terms cannot be a function of individual personality exclusively, but is a complicated entity which is influenced by three levels of phenomena: (1) what goes on emotionally inside the individual (2) what goes on between this individual and his significant small group and (3) the social and value patterns of the larger community. We have had a unique opportunity to observe the effects of these multiple level interactions on the subjects studied in our investigation.

Many of these subjects came from a homogeneous, well-demarcated, subcultural group. The standards and values of this group are largely patterned upon those of the traditional English upper middle classes of pre-Socialist England. We have become aware of how the values of this group enter especially into the moulding of the personality areas of affectivity and assertiveness in the subjects studied. It is our impression that in this group the expression of positive, tender warm affect is largely discouraged. This is most marked in relation to the males and they suffer from marked degrees of affective deprivation and constriction. Also, free open assertion in terms of negative feelings and action is frowned upon. The emphasis is placed upon unquestioning submission to authority both in the personified form of parents, masters, and teachers, and in the more generalized, abstract, intellectualized form of the precepts that this group lives by. Many of these subjects have made a surface adaptation to these particular demands placed upon them by this environment, but this cannot necessarily be equated with the achievement of positive emotional health. We feel that Hartmann's (5) statement "the nature of the enviornment may be such that a pathological development of the psyche offers a more satisfactory solution than would a normal one," holds true in cases of this sort.

We present the following case which is a prototype of many we have collected in this study in the hope of demonstrating the preceding observations:

Case 3: Subject is a 16-year-old Arts student. He is very intelligent, and shows a strong intellectual interest and desire to cooperate in the study. His attitude is one of cynical aloofness and emotional distance.

His developmental history is one full of severe behavior problems which terminated abruptly upon being enrolled in boarding school at age 9. He was in frequent trouble at home due to what he calls "Stealing." This consisted of taking mother's compact and lipstick and other such items and either hiding them around the home or giving them to his friends. Lying was used to cover up this behavior and this resulted in frequent beatings from his father. He suffered from night fears consisting of fears that people were in his room in the dark, and enuresis. He describes his mother as being very irritable and hence easily annoyed. "She is not always prepared to listen to what others say—few women are."

Father is described as a severe disciplinarian but "honorable". Subject states "He goes by what he thinks is right—but he is reasonably open-minded. If you can prove your point he'll accept your theory."

His views on why his behavior problems ceased almost immediately upon entrance into boarding school are interesting. He states: "If you didn't stop they would be beaten out of you. I think I realized I was wrong and deserved everything I got—and so I tried to stop. The Headmaster was sympathetic. I was probably one of his favorites, and he looked after me by caning me. My rebelliousness and dislike of discipline just went underground. The rebellion was suppressed and became insolence." Despite such ability to describe this process the subject displays his lack of freedom in this area by such further conflicting statements as "I would send my son to a boarding school like that. I think the discipline there is good for people—it improves the character and makes one appreciate discipline better. I think self-discipline can best be developed by being beaten into one. If the child is at home he comes to hate the parents, but if he is at school he can then only hate the teachers."

Subject's major problem areas are now those of handling the suppressed rage and his difficulties in establishing close emotional relationships. Regarding the handling of rage he states: "I don't get angry now—but I must. I don't express it. I don't get seriously angry—I just get annoyed and I conceal that."

His goals in life are "to lead a reasonable existence. To have a happy, moral and religious life. The great thing in life is mental happiness. It can be achieved through several media—religion mainly. One should endeavor to be as good

a Christian as possible—to be honest, upright, and straightforward."

In evaluating such individuals from the standpoint of assessment of emotional health we must become aware of the complications created by the issue of value judgments.

Cultural anthropologists (17) have demonstrated the necessity of taking into consideration the culture-bound values of the observer not only in studying cultures quite different from our own but also in our own culture. In the evaluation of individuals in our own culture and the various subcultures therein, the distortion potential of the values of the observer is insidious. Objective application of values by social scientists, however, is not to be rejected. In our opinion, these values should be a direct outgrowth of an objective empirical ascertainment of the functional design of the human organism. We should be prepared to assert value judgments concerning the various potential capacities inherent in the organism, and which are capable of being stimulated in most individuals, given the proper developmental environment. If the latter is not conducive to such a positive development of capacities we feel there should be no hesitancy in pointing this out.

It is taken for granted that the scientific observer should be aware of the values used, and be prepared to define them and demonstrate the manner in which they affect his evaluation.

Child development experts demonstrate the importance of such value judgments when in response to questions of what is the best way to bring up children, they say: "It depends on what type of child we want to produce."

In the application of values to cases such as that presented above, we should not be expected to express likes or dislikes for these products of such a system. We should be expected to state whether this approach to child rearing is successful in developing satisfactorily the various positive components of personality makeup which we know, or should attempt to know, are potentially inherent in the human organism.

In other terms we should be expected to state whether these individuals are emotionally healthy in the positive sense or whether they have found it necessary to resort to pathological intrapsychic mechanisms in order to come to terms with their environment.

As our work progressed our ideas ripened, owing to greater clinical experience with the problems involved plus a more intimate acquaintance with the literature. From the latter we took what we felt was helpful and from the former we gained the knowledge and the convictions to formulate ideas and approaches of our own. We combined these to develop an operational approach that has been fairly satisfactory—although still tentative and flexible and ready to be improved upon at any moment.

We have found it helpful to develop a classification and rating scheme based on an assessment of the personality at all levels of depth. In doing so we hoped to minimize the unavoidable twin pitfalls of vagueness, on the one hand, and artificial constriction, on the other hand, which are inherent in such schemes.

The works of Erikson (3) and Jahoda (7) have been most helpful in the refining of our ideas to the point of being able to construct a rating scheme.

Erikson has developed a hypothesis of personality development based upon a progression of developmental stages. This has as its basis Freud's theory of psychosexual stages of development but Erikson enlarges on this, adding to it the newer insights of both ego psychology and the results of his anthropological researches. He believes that "each stage becomes a *crisis*, because incipient growth in a particular part makes an individual particularly vulnerable in that part." Our remarks here shall be confined to those dealing with the adolescent stage since that is the age group of the subjects we have been dealing with in our project so far. We feel, in agreement with Erikson, that the stage of adolescence has its own special set of conflicts which must be dealt with. Thus the emotionally healthy adolescent is certainly not one who is conflict-free, but one who handles these conflicts inherent in adolescence in our culture in an optimal manner. These are the ones whose personalities become enriched as a result of weather-

ing these conflicts and not impoverished as is the case in the not so healthy individuals.

Erikson's ideas on the assessment of emotional health are astonishingly close to those of Jahoda—even though they speak a slightly different language.

Jahoda suggests that a combination of the following three criteria be used for the determination of emotional health:

1. "Active adjustment or attempts at mastery of his environment as distinct both from his inability to adjust and from his indiscriminate adjustment through passive acceptance of environmental conditions.

2. Unity of his personality, the maintenance of a stable, internal integration which remains intact notwithstanding the flexibility of behavior, which derives from active adjustment.

3. Ability to perceive correctly the world and himself."

To the latter two criteria we would add the qualifying statements that they should be applied to adolescents with the awareness that their personalities are still in a state of flux and not yet fully crystallized.

The classification and rating scheme developed by us is as follows:

A. Absence of structured psychiatric symptoms.
   Social and occupational adaptation.
   Dynamic integration.

B. Absence of structured psychiatric symptoms.
   Social and occupational adaptation.
   Mild impairment of dynamic integration with mild anxiety.

C. Absence of structured psychiatric symptoms.
   Social and/ or occupational maladaptation.
   Moderate impairment of dynamic integration, moderate psychopathology and moderate anxiety.

D. Presence of structured psychiatric symptoms.
   Social and occupational maladaptation.
   Severe impairment of dynamic integration, severe psychopathology and severe anxiety.

The classification of social and occupational adaptation takes into consideration the points raised by Jahoda in her first criterion as well as those in the definition of mental health formulated by the World Health Organization. It is at this level that the individual s sociocultural setting is evaluated as discussed previously.

The classification—dynamic integration—covers the factors in Jahoda's second and third criteria, as well as those raised by Erikson (3) in his structuring of the phases of development up to and including adolescence. We look for a satisfactory resolution of all the intra and extrapsychic adaptive problems that should have been dealt with by the individual in his development up to the point of examination.

We consider those individuals that fall into groups A. or B. to be within our range of emotionally healthy individuals.

The correlations between the independent ratings of the psychiatrist on the basis of interview findings and those of the psychologist on the basis of Rorschach testing have been surprisingly high.

We feel that on the basis of our ratings, we can with relative assurance label an adolescent as being emotionally healthy or not *at this particular period in his life* with fair predictability of his remaining so in the future.

Because of the peculiar conflicts of the adolescent in our society, as alluded to previously, we feel we cannot go beyond the above in our predictions for future adaptation of those individuals. The individual's psychic balance is so fluid during adolescence that it could take sharp dips rather suddenly—like a ship in a sudden squall—and these defy too definite prognostications. However, in most cases, if the individual's basic adaptation is sound, he will usually come out of these squalls riding on an even keel.

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(Manuscript received June 14, 1957.)

It is the nature of an hypothesis, when once a man has conceived it, that it assimilates every thing to itself, as proper nourishment; and from the first moment of your begetting it, it generally grows the stronger by every thing you see, hear, read, or understand.

—LAURENCE STERNE, *The Life and Opinions of Tristram Shandy, Gentleman*

The method of discovery and proof whereby the most general principles are first established, and then intermediate axioms are tried and proved by them, is the parent of error and the curse of all science.

—FRANCIS BACON

Psychological diagnosis is sometimes based on clinical judgment. This article illustrates an attempt to make these judgments more objective. It describes a relatively new diagnostic technique in which the scoring is objective and does not require extensive psychological training. It may also save time by being a possible substitute for lengthier test batteries.

SCREENING NEUROPSYCHIATRIC PATIENTS BY MEANS OF THE KAHN TEST OF SYMBOL ARRANGEMENT

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WHEREAS the private practitioner in the mental health field can afford to concentrate on the treatment aspect of his patients, often letting the matter of diagnosis go until therapeutic relationships have been well established, the military psychiatrist must consider diagnosis as a primary objective. The reasons are obvious. Diagnosis determines whether or not the man will be discharged, what, if any, pension he is entitled to receive, what disciplinary action should be taken, what treatment should be instituted, and a host of other questions which must be answered in a minimum of time. If the military forces are reduced, diagnosis of mental condition will play an even more important role in that it will be necessary to rehabilitate and retain all highly trained specialists so that each man may pull a maximum amount of weight in keeping the military establishment going at top efficiency. Correct, efficient, and practical diagnostic methods and techniques are, therefore, of continuing interest to the military psychiatry team. It is the purpose of this paper to describe the use of a new diagnostic method which seems to have proven efficiency in assisting in the task of psychodiagnosis in military hospitals.

The Kahn Test of Symbol Arrangement first appeared in the literature in 1950 when Fils (3), in a doctoral dissertation, compared 50 schizophrenics with a like number of normals and found that 30 test responses differentiated the two groups at the .001 level of confidence—a very satisfactory statistical differentiation. Kahn had standardized the test several years preceding this study using over 2000 persons (13). A number of investigations followed to determine how well the instrument could identify organic brain pathology. The results were not only favorable but somewhat amazingly so. In a validation and cross-validation study Kahn succeeded in screening patients with CBS from matched non-brain-damaged groups with practically no errors except in higher age ranges (8, 9). Brodsly found differences between an epileptic and a non-epileptic group (1). But perhaps the most compelling argument in favor of the test in terms of its screening capacity came from research in which the Applied Mathematic Branch of the Wright Air Development Center, participated.[1] Using only the *symbol pattern*—a number and a series of letters—which the test provides in lieu of a test score, the mathematicians were able to correctly classify 71.8% of an unknown population consisting of 84 normals, 30 neurotics, 85 schizophrenics, and 91 organic psychotics (13). Furthermore, the Clinical Manual which accompanies the test materials, gives evidence regarding the test's ability to differentiate between normals, neurotics, patients with character and behavior disorders, borderline schizophrenics and psychotics (13).

[1] Dr. H. Leon Harter, Dr. Paul R. Rider, and Mrs. Mary D. Lum.

These results were further buttressed by an investigation completed in which the present authors participated (16). In this study an unselected group of 48 airmen patients were classified into four categories by forced choice, using the patients' Kahn Test symbol pattern and no other data. The categories were: neurosis, character and behavior disorder, organic brain damage, and schizophrenia. The results showed that 38 out of the 48 patients, or 79.17% were classified correctly as compared with an expected 25% (since there were four categories) if the symbol pattern were of no value in classifying the patients. In this study all of the four patients with cortical lesions due to cerebral trauma were correctly identified which could have occurred once in approximately 80 million if chance alone were operating (16). Other studies confirm these findings (2, 4, 6, 7, 10, 15).

From the military point of view the Kahn Test has the following advantages:

1. Symbols used on the test include those associated with the military service: an anchor (Navy), three stars (Army), wings, (Air Force). The total of sixteen objects are easily manipulated, the directions (consisting of asking the patient to arrange them five times on a segmented felt strip) are simple and the test scoring is largely objective. Testing takes only fifteen minutes (12, 13).

2. The test is easily scored in approximately three minutes and the score requires a minimum of training in psychology. No highly trained personnel need be used.

3. Since the test includes both verbal and manipulative tasks, scorable responses are elicited even if the patient is verbally blocked or has language difficulties.

From the point of view of the military clinical psychologist, the following facts are pertinent:

1. The average psychological test battery used for arriving at a diagnostic appraisal requires a minimum of three tests—usually the Rorschach Test of Personality Evaluation, the Minnesota Multiphasic Personality Inventory, and the Draw-A-Person or the Sentence Completion Test. Often, the Thematic Apperception Test, Word Association Test, Weigl-Goldstein-Scheerer Tests are given additionally. This represents anywhere from four to eight hours of psychological testing. In an appendix to his doctor's dissertation Fils claims that the Kahn Test of Symbol Arrangement is capable of yielding by its sole use, essentially the same information as was supplied by a whole battery of other psychological tests (3).

2. Fils' contentions gain support from the investigation by the present authors who found the following percentages of agreement between the Kahn Test, evaluated and scored separately, and the usual psychological battery consisting of at least three of the tests mentioned in the above paragraph:

Percent of Agreement Between Kahn Test and Battery

	Kahn Test		
	Diagnostic Summary	Pre-morbid Personality description	Prognosis of adjustment
Battery	89	77	82

These findings indicate a relatively high percent of agreement between the Kahn Test and the total testing exclusive of the Kahn Test. It should be noted that the study does not attempt to determine whether the Kahn Test or the Battery was correct in regard to those cases where disagreement between the two existed. The point is that with such high percentage of agreement it may be possible to dispense with the battery on those occasions when time is a pressing factor. The Kahn Test of Symbol Arrangement takes only 15 minutes to give and a few minutes to score.

Finally, by providing definite formulas which are applicable to specific nosological groups, the psychologist has, at last, his chance to attempt to be scientific. Most of the scoring criteria are completely objective, others follow definite rules such as those used in the evaluation of intelligence test tasks. The danger of projecting oneself into the test situation, and factors such as the "halo effect" and counter-transference are thus minimized. The symbol pattern method of scoring makes it possible for the test results to be blindly evaluated by a sorting technique in which the scorer needs neither knowledge of psychological methodology or of the patient's history (13). It is believed that the test's efficacy relative to other

psychological techniques can only be explained by the fact that it offers a radically new approach to the problem of personality evaluation as pointed out by Kahn and Murphy (11). There is a priori evidence that the materials used in the test have meaning by virtue of the fact that the shapes used were sold in hobby shops throughout the United States for their meaning and appeal to the general population. The objects are: three hearts, three stars, three dogs, an anchor, circle, cross, parrot and two butterflies. All of the objects differ in color, shape, thickness, size and translucence. The patient is required to arrange the objects on a felt strip composed of serially numbered segments. He does this five times under varying conditions. Finally he sorts the objects on the back of the record sheet which is supplied by the publisher (12).

The fact that we are dealing here with concrete and clearly recognizable shapes—such as hearts, stars, a cross, etc. rather than with ambiguous stimuli situations such as offered by the Thematic Apperception Test pictures or the Rorschach blots, makes it possible to obtain clear-cut and relatively reliable answers. For example, if a patient shows obvious dislike for the heart-shaped objects after having stated that they represent "love" or "sweetheart," and if he, later, associates these shapes with the word "hate" in the sorting task, we have some valid reason for assuming that there is something disturbing him in the area of his affective relationships. Other tests fail to offer such unambiguous insights. When considering this with the fact that the symbol pattern which the test yields offers us practical diagnostic formulas, one is led to the conclusion that the symbol arrangement technique is a valuable tool in the task of screening and evaluating neuropsychiatric patients in military hospitals and mental hygiene clinics.

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(Manuscript received October 18, 1957.)

CONFERENCE ON TRAFFIC BEHAVIOR RESEARCH

A Summary of the Conclusions of a meeting Called by the President's Committee on Traffic Safety at Williamsburg, Va., February 24-28, 1958

Background. The conference was convened "to generate new ideas for traffic-safety research, particularly in the field of human behavior." The need for research on traffic safety springs from the serious and complex nature of the problem and from the inadequacy of existing knowledge. Despite the extensive work already done by individuals and organizations, the current supply of scientific findings is insufficient to provide a firm basis for public policy. The problem of traffic safety, like other important public health problems, demands large-scale support from private and public sources.

Research Strategies. Encouragement should be given to basic as well as applied research. From advances that have occurred in the solution of other problems (in the fields of health and industry, for example) we are convinced that more effort must be devoted to basic or fundamental research.

The phenomenon of highway accidents cannot be fully understood without considering it in the context of routine traffic behavior; hence research must include the study of normal as well as abnormal patterns of behavior.

The nature of the problem makes it necessary that research be conducted both in suitable laboratory situations and in selected field situations (e.g., communities, sociocultural groups, and other populational segments). In many instances field research can be effective only with the cooperation of civil authorities and informal leaders.

The promotion and support of research should be such that findings of the several appropriate research disciplines may be available for integration and application to the problem at hand.

Research efforts both by groups and by individual workers should be encouraged.

For best results, research should be supported more on a long-range program basis than on a short-term project basis.

To the extent possible, research should be conducted by organizations with a maximum of research talents and a minimum of inappropriate pressures.

For operating purposes the conferees divided themselves into three groups concerned respectively with the following topics:

1. A systems approach to traffic flow and driver behavior.
2. The psychology of driver behavior.
3. The social context of the automobile, its use and regulation.

It is recognized that the divisions are somewhat arbitrary and not necessarily mutually exclusive.

I. *A Systems Approach to Traffic Flow and Driver Behavior*

A vigorous research effort aimed at measuring actual traffic dynamics and actual driver-car behavior in various traffic situations is required in order more clearly to delineate dangerous situations and to indicate ways whereby new road and automobile design and modified traffic rules can increase traffic flow and reduce traffic hazard. This research effort should include at least four coordinated activities:

(1) recording and analysis of the actual flow of traffic in various localities at various times and conditions of stress;

(2) using the parameters of traffic dynamics obtained from (1), simulating traffic flow by digital computing equipment so that special situations can be studied in detail and the effects of experimental modifications can be measured quickly and safely;

(3) equipment of special cars to observe the behavior of the individual driver in relation to his traffic environment as he drives along, to supplement the mass observations of (1);

(4) development of equipment to simulate the traffic environment of the driver,

to supplement the observations of (3) and to extend them into situations of stress which might involve danger of accidents. Such a driver-environment simulator might also be of use in determining the efficacy of training or testing programs and in determining the effects of alcohol, fatigue, and other factors on driver behavior, though careful correlation with the results of (1) and (3) would be required to insure realism of the findings.

The quantitative analysis involved in the program just described presupposes a proper selection of a great number of statistical indices. However, what these are can be decided only as the program develops. Therefore, the program needs to be supplemented by a methodical development of proper statistical techniques. The development of such techniques, however, involves an additional dimension of statistical research, what amounts to a demography or epidemiology of traffic behavior. This should be carefully coordinated with a continuing nationwide standardized statistical reporting system (corresponding to vital statistics and other census statistics).

The following types of research design pertain to the topics mentioned above.

1. *Observation of Mass Traffic Flow.* The position, velocity, and acceleration of a car should be measured over about 1000-foot stretches of road, in relation to the relative position, velocity, and acceleration of the neighboring cars. A camera or cameras, placed about 100–200 feet above the road, with photos taken about every second, could produce the required accuracy (about 2 feet in position). Long sequences of such shots should be taken at different locations and under different conditions of traffic stress (day, night, dry, wet, presence of police, etc.) The pictures can be projected and the vehicle positions transcribed by "assembly line" methods to punched cards or tape so that high-speed computers can calculate the changes in velocity and direction of each car as a function of the relative positions, velocities and accelerations of the neighboring cars and of the road conditions. From these computations the probability distributions of car-driver reactions can be tabulated, for analyses in respect to the effects of car and road design and also as required input for the traffic flow simulation of item 2. On the basis of first observations experiments can be devised to clarify various unclear points.

2. *Simulation of Mass Traffic Flow.* Using the dynamic parameters of driver-car behavior, a large digital computer can be programmed to simulate traffic flow, to compute positions and velocities of a population of cars on a stretch of road, a sequence of intersections or any other portion of a road network. The variability of car-driver behavior can be included and the density and speed of input can be varied to simulate various conditions of stress. Total flow can be calculated and the nature and number of accidents and near-accidents can be recorded. After the situations observed in (1) are duplicated, to check the realism of the simulation, many simulated experiments can be performed. The frequency of near-accidents and accidents can be measured as traffic stress, of various sorts, is increased, the effect of more strict enforcement of traffic rules could be tested and many other things could be tried out quickly and safely.

3. *Observation of Driver Behavior.* To complement the measurements of gross behavior of car-driver described in (1) detailed recordings of individual driver reaction to traffic environment should be made. To do this a specially instrumented car or cars must be used which can be driven through various traffic situations, recording what goes on around the car and what corresponding reactions of the driver occur. One possible method of implementing such a program would be an all-round-looking camera which would photograph the car's environment in a circular strip along which absolute direction could be recorded. In the circle within this strip, the film could record time, brake and accelerator position, steering wheel position, car acceleration, and any other appropriate phenomenon. Here again photographs could be taken about once a second and the layout should be designed for easy (or perhaps semi-automatic) transcription to digital computer input. Possible results of these meas-

urements could be more detailed knowledge of car-driver reactions to traffic environment, to supplement (1) and required by (2). Other results might be greater insight as to possible changes in car and road design to improve these reaction behaviors. Analysis of these observations will be of help in designing and using the driver-environment simulator of (4).

4. *Driver-Environment Simulation.* A number of complex problems, such as piloting aircraft, have been studied by the development and use of devices that *simulate* dangerous or difficult-to-observe environments. These devices depend on the fact that humans "fill in" much of the detail of the visual field, for example. Just how much "realism" is needed to obtain realistic behavior in the simulation is not known, but the problem can be solved (expensively!) by preliminary experimentation.

The minimum requirement for such a simulation would be an automobile body with seats, instrument panel, and controls such that a subject could enter the automobile, start the car, pull away from the parked position, into a stream of traffic and drive for several hours seeing and hearing stimuli that behave as if they came from the real environment. The simulator must react appropriately to every action initiated by the subject. In particular, he must be able to have "accidents" in the simulator.

It should be emphasized that we are proposing a kind of simulation here that has not been attempted previously because of its complexity and cost. For example, each "vehicle" in the field of view must react appropriately to an action on the part of the driver. It is impossible to program such appropriate reaction into a computer—one must contemplate using a staff of human "stooges" to apply predetermined strategies, (which may be quite complex) rather than predetermined responses.

If it were possible to build and finance the operation of such a large-scale simulation device, what would it be used for? It seems to us that such a large driving laboratory might be used first of all to study in a quantitative way what drivers actually do in a variety of normal driving and stressful driving situations. It should help to answer the question as to which of the many aspects of real driver behavior that might be measured is it profitable to try to measure. In addition, the simulator would make possible studies of the effects of drugs, fatigue, motivation, rest pauses, etc. on driver performances.

The problem of cost should be one of the first considerations in contemplating this proposal. Simulation research, particularly where large numbers of people are involved, is an expensive undertaking.

Statistical and Methodological Problems. The available statistics on accidents presuppose several systems of "taxonomy." For example, the 12 types of accidents now used in standard reports presuppose a classification into non-collision and collision and further sub-classification of these largely according to types of obstruction (if any) encountered. On the other hand, some researchers have proposed alternative classifications. For example, one is based on failures to follow a prescribed path and on deviations of the relative direction and velocity of the car (a quantitative vector variable). Still another classification emphasizes type of road, departing from previous conventional classification into rural and urban (which has lost much of its meaning) and arriving at more relevant classification of thoroughfare types.

Many other taxonomies exist which, it is supposed, are suited to the various purposes for which they have been designed. Clearly, one of the first problems in basic research on the general aspects of traffic behavior is to design a taxonomy particularly suited to the types of problems to be considered. This should be done with reference both to types of accidents and to various indices of severity, and any other quantitative measures from which guidance may be obtained toward an understanding of general traffic properties.

There is a serious and relevant problem to be decided in that little is yet known as to whether the individuals who have one or more accidents of a given severity differ essentially from those who do not. To say it in another way, is the accident rate this

year higher for the subgroup of persons who had an accident last year than for the subgroup of those who did not? If it is higher, by how much and for what causes?

If the rate of repetition for accidents is higher than the accident rate, but not by much, the problem of identifying the accident-prone driver from his accident record is very nearly as hard as identifying him before he has had an accident. Barring all drivers who have had more than a critical number of accidents will in that case not appreciably reduce the over-all rate in the future. In fact it is quite probable that almost all accidents involve normal individuals (i.e., those whose "accident-proneness" [in accidents to be expected per mile] differ at most by a few percent from the average.)

The suggestion arising from the analysis underlying this discussion is that sophisticated mathematical and statistical analysis is required to determine the extent of "accident-proneness" in the driver population. The difficulty of determining accident-proneness in the individual is extreme because of the fact that accidents are such rare events.

Summary—The goal of the research program outlined here is envisaged as primarily development of quantitative indices of road behavior which may be considered "normal" within specified conditions. Actual road behavior of different individuals or of an individual over a period of time will exhibit distributions of these indices which, on occasions will pass outside of the "normal range." Whenever this happens in conjunction with a concatenation of certain environmental conditions, a "near-accident" of various degrees of approximation will occur, which in the extreme case will become an accident of a given severity. Since near-accidents occur with a frequency exceeding by several orders of magnitude the frequency of actual accidents, the focusing of research on these situations should alleviate the difficulties associated with the statistical study of rare events.

It seems desirable to have a quantitative study of a continuum ranging from safe to disastrous patterns of behavior in which observational and experimental findings would be given an appropriate theoretical base.

II. *The Psychology of Driver Behavior*

Driver behavior as a system can be described as being composed of three elements: input, organization, and output. These need not be investigated separately, but are so considered here for convenience.

Input is of external and of internal origin, arising in the external environment or from within the organism. The input is integrated and made meaningful by the organization factor, which selects that which will become output from available motor skills.

The input for driver behavior requires intensive investigation. We need to know what constitutes effective input and the variance of its effect on output in the widest possible sampling of drivers.

The organization factor may be considered on two levels. The first is more conceptual or symbolic, requiring higher integrative processes, with a high degree of driver awareness. The second level is more automatic, requiring less awareness.

We need to know how the level of less awareness may be utilized to replace the probably highly variable activity of the level of greater awareness, and under what conditions. How may "big brother" controls substitute even for the more automatic driver behaviors?

It is of special importance that the entire area dealing with attention be investigated, with particular reference to different driving conditions, and in a wide sample of drivers.

Motivational systems account in large measure for the functioning of the organization factor. What are these systems? How do they develop? How may they be utilized in teaching safe driving? How are they influenced by educational measures and by social pressures, especially by the peer group? What is the role of anxiety, and how does it relate to enforcement on the one hand and good driving on the other?

What constitute effective rewards and punishments in the development of driving skills? How may these be incorporated into an educational program which has as its

end point training for effective and safe driving under *all* road conditions?

Output, especially conceived as overt driver behavior, requires objective measurement. For this, instrumentation as well as subjects are necessary. A driver simulator is essential but is not the only device or technique to be used.

Research must not be limited to the driver; the passenger and the pedestrian require the same intensive study.

An elaboration of these descriptions and suggestions follows.

A. *Input.* Input is both external and internal.

1. *External input* refers to sources of stimuli outside of the person in the immediate time environment of the driving situation. Research in this area may be concerned with questions such as the following: What are the perceptual configurations which make up the driver's external input? Particularly, how are inputs which signal actual or potential danger perceived? What are the variations in perception of the same stimulus pattern which can be found among different drivers? Many hypotheses can be generated from knowledge of perceptual variation. For example, we may hypothesize that the number of perceived crises varies directly with the amount perceived and its meaningfulness. Similarly, it is interesting to speculate how the number of actual crises is related to the number of perceived crises. We can even hypothesize, more broadly, that there exists an optimal perception level. In any event, the over-all question is to establish the relationships between perception and the criteria of good driving. This implies, among things, the introduction of different ways of observing—either experimentally or by selection from a population. In sum, we need research into techniques of measurement and description of external input. This should include correlations with physiological measures (EEG, polygraph, etc.), gross behavioral measure, measure of objective relations among input elements, etc.

2. *Internal input* refers to all information arising from the person. This includes that which comes from the body as well as present psychic states which may have arisen from past experience. We need research into techniques of description and measurement of internal driver input; for example, set, perceptual tendencies, present habits of perceiving, visceral sensations, drowsiness, etc.

B. *Organization Factor.* The organization factor is the integrator of these inputs. It may be regarded here as including habit systems.

1. *Levels of the organizing process and their modification.* There are two levels of organizing processes. The first is on the conceptual or symbolic level, requiring attention, highly integrating, and requiring *much* awareness. There is generally a progress during the learning of driving, from the first to the second, i.e., from more to less awareness, for any set of acts. Furthermore, the degree of automaticity finally achieved for various sets of acts varies in a driver from set to set and varies from driver to driver. We would like to define various conditions leading to different degrees of automaticity. This definition should come from research. Therefore, research into judgment or decision-making behavior is very important. It can guide research on automatic controls which will substitute adequately for the driver's failure.

For example, it might be possible to develop more "big brother" controls than we now have in order to make certain processes of the complex type less complex where they are dangerous, and processes of the more automatic type less dangerous. A "big brother" control might be one such as the automatic signal telling of speed beyond a present level, or on an express highway where it is dangerous to stop, a gong telling of impending gasoline shortage, or radar control warning of a vehicle too close in front.

We would like to carry out studies on the distribution and loss of attention under various driving conditions. Because it is well known that attention cannot be continuous for a demanding task, it may be that certain types of people, certain types of traffic conditions, certain conditions of the individual, certain habits of behavior, lead to different degrees and numbers of attention lapses.

We would like research evidence on these matters.

The task of formal driver education is to teach the pre-driver how to operate efficiently at both levels of complexity of organizing process. The driver training teacher is concerned with initial learning functions, and not with review of learning functions of experienced drivers. There is some evidence that training of fleet drivers may improve their accident experience. Can this question be answered more definitely? Can the ordinary driver's accident experience be improved by review training? How long will such training be effective?

What would be the effectiveness of advanced driving schools where a man could get a certificate or badge or special license plate? This might be an incentive to reach and an incentive to keep. Since most people think they are good drivers, this may be a means of showing them that there are different levels of driving excellence and that what they considered good driving might in fact be dangerous driving indeed. Certainly the effectiveness of the existing British organization of this type should be measured.

The general problem of driver education is receiving some attention, but should probably be examined with much greater care, in an attempt to find out something of the effect of all the various elements of training programs. This means, that evaluation research is of great importance not only as to individual curriculum components, but also as to their distribution in the training courses. Special attention should be paid to training in emergency measures and to attitudes toward the goals of driving.

2. *Motivation and its modification.* The motivation systems in driving seem to be multiple. We would iike to know what these systems are. We would like to select reinforcements for driving so that rewards for the acts in driving are more compatible with certain motivational systems than with others. For example, how is the efficiency of automatic behavior—or for that matter, behavior of any degrees of complexity—affected by changes in anxiety? By others? By one's family? For example, after two moving traffic violations, what kind of behavior characterizes the driver if he is threatened by imminent loss of license? This can be related on a theoretical level to the effects of differences of punishment gradient. What is the relative effectiveness of differences of suspension of license as contrasted with fines?

In practice, good driving habits may be rewarded or actually punished; likewise, bad driving habits may be punished or actually rewarded. We need to study the kinds of reinforcements that exist. We need to study the effects of changing these. For example, an experimental estimate can be made of how satisfying certain behaviors are. How will satisfactions fare when placed in competition with certain punishing factors? Here we can profitably investigate the motivational value of certain risks vs. certain punishments. This is an especially fertile field for investigation because some evidence suggests that while knowledge of increased risk does not effect behavior, other knowledge does. We want to know how much information drivers have about driving risks, and what would happen if they had more information.

All of the above—indeed all motivational studies—must be carried out against the background of early learning and reinforcement of various attitudes toward society, morality, one's peers, etc.

How do all these attitudes develop?

In this connection, it would be very appropriate to study the development of ethical understanding and behavior, feelings of responsibility toward society, toward the family, and in a grimly logical way, toward the entity such as a peer group whose ethics demand what the outgroup regards as unethical. How ethical is it to reward someone for doing what he should do anyhow? How can feelings of responsibility in other spheres be channeled to act just as strongly in the person's relations with other drivers?

In all of these areas of investigation, it will be necessary to select certain subjects as samples of the population as a whole. The sample must include members of various socio-cultural groups and subgroups, males and females, different age groups, etc., as

well as drivers of different abilities and experience. It is important that drivers who have participated in accidents of specified degrees of severity be available to aid us in finding out what actually happened in these accidents.

It has been asked what might happen to the aggressiveness or other anti-social impulses assumed to be involved in some accident-producing behavior if this outlet were no longer available. Might this not be like a conversion symptom, such as hysterical paralysis of the leg? If modified by some peripheral means, this might be the case; however, if modification were made in basic attitudes, might not the corrective effect be more complete and permanent? This must be investigated; if morality is not instilled in early life, if the bases for responsible behavior are not early established, may it not be too late by the time driver age is reached?

Perhaps a searching investigation must be made of the values of American civilization and of our attitudes toward the automobile as a public institution, perhaps even a longitudinal study of this concept. If it is important enough as an influence, it may be time to examine the whole structure of the value systems of the country. The next generation is the one which our education will influence.

It was brought out previously that there exist motivational systems at various levels and of different kinds. It would be well to relate the different kinds of motivational systems in our drivers. These problems can be attacked experimentally, either in live car situations or in a simulator.

Parenthetically, the amount of profitable research which can be done in a simulator is so great that it would not be amiss to recommend expansion of the program of producing a simulator once the prototype has been developed successfully.

One interesting attitudinal question relates to the popular concept that accidents are inevitable. Means should be examined to discover the possible existence of such attitudes and of means of changing them if they exist.

C. *Output.* Output includes all the behavior of the individual as a result of initial input, feedback from other behavior, and intermediate organization. We cannot divorce the relationship of output and organization, since different outputs will result from alternative decision-making processes of organization.

We would first like to describe the most efficient motor behavior of the human in general terms, to which questions of automobile behavior can be referred for comparison. For example, the push-pull steering of the Cornell-Liberty car is more compatible with movement actuated by muscles capable of fine control than is the conventional steering wheel, all other things being equal.

Motor behavior can be measured by instruments now available, and some attempts have been made. But the full potential of the methods available to us has not been exercised, principally because of lack of funds. Research in terms of motor analysis is of importance.

D. *Research on persons other than the driver.* In the areas of attitude, enforcement, motivation, perception, and training, the pedestrian is as fit, and perhaps a fitter, subject for study than the driver.

III. *The Social Context of the Automobile. Its Use and Regulation*

In planning research, the problem of auto accidents can profitably be viewed from various perspectives or frames of reference, including:

(*a*) The *place of the automobile* in American life and in the different subcultures that make up America: the ways in which the use and meaning of the automobile are related to other aspects of our total pattern of life—historical, political, legal, economic, recreational, technological, familial, etc.

(*b*) Highway driving viewed as a *social activity* or dynamic social game involving the role of the driver as affected by his perception of, and interaction with, persons playing other roles on the highway scene (passengers, other drivers, pedestrians, law officers, etc.) and requiring conformity to a socially defined set of rules whose violation constitutes anti-social behavior.

(*c*) Use of the automobile considered as

an *economic activity*. So considered, automobile traffic involves choosing between benefits and costs (including indirect and nonmonetary benefits and costs) and how they should be allocated and who is to bear their burden. An automobile trip by its very logic tends to differ from an ordinary economic good in several respects—e.g., there are numerous external economies and diseconomies; consumer preferences interact; there is uncertainty, risk, and misperception of risk; there are other kinds of consumer ignorance; there are social overhead costs. The usual market mechanism fails in this area to allocate the burden and to encourage the economizing of human life, time, and money. Research should aim at specifying an "ideal solution" and, once this is done, uncover the most profitable next steps public and private policy must take. The problem of the economies of traffic safety is akin to the economies of public health and public goods in general; for instance, schools, courts, defense, etc.

These lines of research are suggested:

(*a*) The development of more valid and reliable measures of traffic safety than are now available, to serve as criteria needed in subsequent research.

(*b*) Analysis of the relationship between serviceable indices and meaningful subdivisions of the entire population (e.g., divisions by social class, age group, educational status, kind of driver training, etc.) so as to generate new hypotheses for explaining highway accidents and for attempting corrective actions.

(*c*) Testing promising hypotheses, techniques, and devices—e.g., varying kinds of penalties, different balancing of punishments and rewards, different types and courses of driver education, varying intensities of enforcement, devices for reducing driver anonymity or increasing communication to and between drivers, etc.

(*d*) Systematic examination of the combinations of factors associated with the acceptance and the non-acceptance of effective safety measures by organizations, communities, and groups.

(*e*) Study of the possibilities and consequences of modifying the traditional allocation of responsibility for traffic regulation and safety activities among governmental and other agencies.

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COMMENT

Two Heads Better Than One?

THE following editorial is reprinted by permission from Science, 25 April 1958, 127, 933:

"A characteristic feature of the perennial call to action in behavioral science is the project for producing a unified theory of man and society by bringing together experts from various special fields. If the experts are given an extended opportunity for close communication, so the proposal runs, then they will successfully pool their separate bits of knowledge. Much hope is placed these days on team efforts, and there are occasions both in science and in other enterprises when a team effort has many advantages. However, there are also occasions when knowledge is not additive, when a team is no better than its best member.

"The motivation for seeking a unified theory of behavior arises in part from a comparison with physics. The study of man and his institutions has been conducted for about as long as has the study of inanimate matter, but with somewhat less success. One peculiarity of physics is the way it develops around separate centers which are then combined to form comprehensive theories. But in behavioral science, despite the development of impressive techniques for measuring intelligence, attitudes, and other quantities, and despite extensive qualitative studies, efforts to effect the same kind of unification as is found in physics have not met with the same kind of general acceptance of results.

"Although a comprehensive theory of behavior may be most desirable, we question whether a team project offers any special promise of success. And, by continuing the comparison with physics, we may be able to suggest why not. Consider, first, a rather different sense in which two sciences may be pooled—namely, the use of one science as a base of operations from which to assist the advance of another. An example from current research is the use of rockets to advance the study of the upper atmosphere. Here the team approach is clearly useful, but unfortunately there is no real unification. In fact, expert A can make use of expert B's knowledge only because A does not need to know what B knows. Thus, the man who launches the rocket need not know anything about its payload, and the man who studies the upper atmosphere need not know anything about how his instruments got up there.

"Now consider the problem of uniting the parts of science to form a comprehensive whole. The temptation again is to identify the men who know the parts and to unite *them*. But now this operation makes less sense, for to make the separate pieces of science stick together requires the cement of new scientific knowledge. Expert A can no longer make use of expert B's knowledge, because A and B both ask the same question and neither knows the answer. Thus, suppose that in the middle of the 19th century a group of experts had been formed to tackle the problem of unifying the sciences of electricity and light. If the group had included James Clerk Maxwell, then surely it would have been successful—but then how necessary would the other members of the group have been? If the group had not included Maxwell, then to produce the electromagnetic theory of light, the group would have had first to come up with the equivalent of Maxwell's revolutionary hypothesis of displacement currents.

"No doubt, at every level, from the family to international relations, there is a great need for the applications of a successful behavioral science. But without being branded as hopelessly old-fashioned, may we suggest that there are other ways to spend money in this science—ways which, if less dramatic than team projects of uni-

fication, are also more likely to succeed? Two heads may be as good as one, but they are not always better.—J.T."

* * * * *

A number of issues are raised in the editorial reproduced above. To deal with any one of them to the extent it deserves, we might need the space of an entire issue. However these questions will always be with us, and we are sure the pros and cons will continue to be discussed in scientific journals. We will therefore confine ourselves to a reply as cursory as the challenge.

J. T. makes a distinction between the pooling of knowledge, so as to use one science as a base of operations for another, and unification of knowledge to form a comprehensive whole. The examples he gives show that the first type of "pooling" underlies complex technological enterprises rather than science itself. The *result* of the technological enterprise may be scientific knowledge, but this is incidental. What was pooled (or tapped) was technique. There is no reason why such a pooling of techniques may not operate in the pursuits of behavioral science. The biologist may prepare for the psychologist a certain strain of experimental animals; the psychologist may prepare for the sociologist a questionnaire aimed at probing certain attitudes; the statistician may analyze the sociologist's results; etc.

In these situations the teamwork is no more than a division of labor and coordination of skills. Such "teamwork" is as justified in behavioral science as in any other complex organized activity.

It is obvious that the more important question concerns the value of teamwork in the genuine unification of knowledge rather than a pooling of techniques. J. T.'s point seems to be that the successful unifications in the past were typically products of exceptional single minds (e.g., Maxwell's).

It is also true that in the past important technological inventions usually sprang from individuals' inspirations. This was the case when technology and technological progress were still peripheral in the life of society. When these matters became central, inventions and developments became every day occurrences, products of organized, methodological search usually by teams of workers.

When science was a peripheral activity, only individuals could be counted on to contribute. As science becomes central, it is inevitable that at least part of the scientific endeavor becomes an organized and systematic search. Certainly the role of the individual, especially the talented individual, should not be submerged whether he works in a group or not. If teamwork in science inhibits individual creation, to that extent teamwork is a liability. But one suspects that a self-selection is bound to operate, that really talented individuals who would be hampered by cooperative enterprises will continue to work alone. It would be mistaken to *insist* that science become a cooperative endeavor, as much so as to ignore the potentials of group endeavor.

Now for the oft-repeated warning (explicit or implied) against imitating physics. Much depends, of course, on what is abstracted from physics to "imitate." Is it rigorous experiment? Is it the predominance of operational definitions? Is it the use of mathematical deduction? Certainly in our day these methodological principles are no more the exclusive property of the physicist. They belong to science, not just to physics. If there is to be a behavioral science, certainly it must have some methodological connection with the other sciences. The principles listed seem to be sufficiently general to justify the assumption that any group of workers pursuing scientific investigations will have at least this conceptual repertoire in common. However their actual experiences with these principles will be different. Each discipline has its own standards of experimental rigor; its own established "operations," on which definitions of concepts depend; and in some cases its own favorite logical schemes and mathematical models.

The Babel of specialists' tongues sometimes creates such a cacophony of noise that meaning is almost drowned out. The idea behind interdisciplinary teamwork is

to forge a group of experts into an instrument for the development of a common language, which is so badly needed not only for the behavioral sciences at large but even for the subdivisions of a given field, such as anthropology and psychology. If the group makes the development of such a scientific Esperanto one of its preliminary goals, it can bring to bear on this goal the same human skills of communication which have always produced a common language whenever people have to live together.

The first important step toward unification thus suggests itself: starting with the fundamental language of science, which all scientists have in common, to try to extend the range of its common language. This cannot be done in a weekend conference but only by sustained effort. That is why it is important to have permanent teams, and that is why it is premature to call attention to the erratic nature of the results so far obtained.

It is well understood that in all the fields of science some persons are more expert in method and others in content or experimentation. A theory group made up of several persons can bring these various skills in a given field and across fields together so that a mathematician can combine with a philosopher of science, an experimental physiologist, and an encyclopedist in the literature of psychology to produce and evaluate generalizations which none could effect alone.

Because specialists have been ignorant of advances in other areas, the same discovery has often been made over and over again in different frameworks. Examples are general principles of organization, of conflict resolution, or of competition and cooperation, which apply to several fields of behavioral science. Group discussion can frequently demonstrate the applicability of ideas developed with some sophistication in one area to an entirely different area where they have not previously been applied. The recognition of isomorphic logical schemes would serve to reduce the waste of effort involved in independent rediscoveries of concepts.

The rapid acceleration of knowledge seen throughout all science applies to the diverse fields which comprise behavioral science. Relevant literature constitutes an information input overload beyond the capacity of most specialists. For example, it has been calculated that if an expert in group dynamics read at the rate of 300 words a minute 24 hours a day seven days a week for a lifetime he would simply get farther behind in the literature. The task of the generalist is, of course, orders of magnitude more difficult. In this new situation in science new social inventions are called for. One of these may be a sort of "group mind" in which behavioral scientists from different overlapping fields act as pools of knowledge for their colleagues, bringing to the group thinking a sort of preorganized ready reference system of relevant materials from their specialty areas. More than an abstracting service or a documentary computer, they can tailor the information they provide to the specific framework of the problem before the group.

It is not denied that the hope of ultimately providing a unification comparable to that of physics is entertained by behavioral scientists who work in teams. It is admitted also that unification "by force" is impossible. At best, unification will come "in spots," and there are a number of such hopeful spots, as is evident to the sympathetic readers of this journal and to participants in serious interdisciplinary endeavors.

We tend to forget that many areas of research which are already unified have achieved unification only recently and (with all due respect to the genius of individuals) largely as results of interdisciplinary cooperation: biochemistry, physiological genetics, psychophysics. What is the score in behavioral science? It remains to be seen. But we cannot at this time reject the opportunity of applying the insights of biology (the notion of organism, its structure, function, and evolution) to more general types of behaving systems (the community, the institution, the society); nor the insights provided by generalized mathematical models (dynamic systems, distributions determined by stochastic pro-

cesses, information-theoretical, or game-theoretical formulations) to all behavioral situations which can be cast into such models. Nor should we summarily dismiss the application of psychodynamics to cultural anthropology, of economics and sociology to history, of linguistics to the psychology of concept formation, or the latter to an empirical study of value systems, without a thorough investigation of the fruitfulness of these ideas. For such investigations, most of which will require teams, time, effort, and funds are required.

To ask where the money can be most "profitably" spent is not very relevant in the context of basic research. We are not faced with an array of slot machines and a problem of determining the one with the biggest pay off. Nor is it a matter of finding the "right road" but rather of exploring various unknown areas and approaches. In such situations, a diversity of effort promises the largest long-term rewards. Let us therefore encourage both the talented individual investigator and the groups of individuals who feel that they can work best together. Let us remember also that creative teamwork has had its lushest development in the United States, an indication, perhaps, that this sort of growth is typically an asset of our cultural soil. Let us not neglect it.

—ANATOL RAPOPORT AND
JAMES G. MILLER

M. A. Kaplan's *System and Process in International Politics*[1]

This volume furnishes additional evidence that, of all the social sciences, political science is one of the most fruitfully in ferment. It may indeed be the most exciting of them all just now. Like the other social sciences, political science remains to a large degree preoccupied with therapy rather than science as such, but in political science preoccupation with both philosophy (in the grand ethical manner) and history (in the most careful descriptive sense) has continued with vigor for longer than in the sister disciplines. Only in recent years, for less than a decade on the whole, has the concern with highly generalized systems of scientific theory as distinguished from ethical philosophy come to the fore. It probably is of small moment whether one deplores or applauds this; in any case Mr. Kaplan's work is a prime example in the new tradition. I feel that this is on the whole a very healthy development, and even those who deplore it need not despair since it does not remotely threaten to sweep the field. In re Mr. Kaplan's work we may all unite in some admiration for a work which substantially advances a serious intellectual concern. I say this despite the fact that in an absolute sense I think this work a failure. Even with the harsh standard that leads me to this conclusion, I would maintain that it is a fruitful failure, and in science one need not ask more even of genius. On balance we must all feel a debt to Mr. Kaplan.

Mr. Kaplan has addressed himself to the field of international relations. There is probably no field of political science which seems currently to have more implications for our daily lives, even our survival. It is a field that scholars as well as an enormous number of practical responsible laymen, some of them of apparent genius, have been preoccupied with down through the ages. Only the hardiest optimist would dare state that we have profited notably in a practical applied sense from the enormous amounts of experience we have been exposed to. Scientifically one searches in vain for a cumulative body of highly generalized theory emanating from all this experience and preoccupation. Indeed it is the proudest boast of some of the most skilled and respected practitioners that we have learned nothing through the ages—that the old masters and classics were, are, and will forever remain the best ones. Maybe they are right, but increasingly young scholars like Kaplan, Snyder, Easton, Apter, and others are rising to challenge them.

Mr. Kaplan starts as far out in the direction of pure science as any of the young Turks has so far ventured. He sets out,

[1] New York: Wiley, 1957. Pp. xxiv + 283, $6.50.

after a slight venture into ontology on the question of "What is politics," to construct a model of the international system or "with possibly greater accuracy, six states of equilibrium of one ultrastable international system" (p. 21). They are: (1) the balance of power system, (2) the loose bipolar system, (3) the tight bipolar system, (4) the universal system, (5) the hierarchical system in its directive and non-directive forms, and (6) the unit veto system. His first major departure from rigorous model building is to point out that these are not the only possibilities and leave the matter hanging in this respect. Definitions of these systems are carefully set up. Only occasionally does one encounter statements that are either true by definition, though not intentionally so, or hazy [e.g., "In any case, two centers of rapidly growing economic potential make for a bipolar international system rather than for a 'balance of power' system." (p. 33)]. Next the international actors are classified largely in two respects: (1) directive as contrasted with non-directive systems and (2) system dominant as contrasted with sub-system dominant. These terms are defined, but it becomes almost immediately clear that the United States (i.e., a democracy personified) is a non-directive, system dominant actor and the Soviet Union (i.e., anti-democracy personified) is a directive, sub-system dominant actor. Up to this point most variables and constants have been carefully stated; the models are rigorous; fat has been cut off the problem.

Now the international actors and their implications are poured through the different states, as it were, of an international system. Here ground is lost. For all of the previous rigor, discussion here proceeds in terms of reference to the unexamined empirical examples, especially the United States and the Soviet Union with others introduced from time to time. But these examples have not been rigorously examined, though I for one find myself usually in sympathy with the allegations of fact about them. Apart from our faith or lack of it, however, the whole point of the exercise is undercut, for by this technique [induced no doubt by the best of all motives, the desire to be relevant in a practical sense] the old bugbear of social science, the difficulty of getting general theory out of a hazily defined system with unstated implicit variables, stalks the stage again. Even so the work has merit for it is not difficult to see when and where this has been done.

Another of the major problems posed by this work is the tendency to think of concrete membership units as actors. To some extent this is a function of the extent to which this work and some of the others in the field have been influenced by the work of Talcott Parsons. But the concept of an actor in the Parsonian sense is a special abstraction in itself. The concept of a state or nation or any other social system as an actor is an abstraction always different in kind from the other, and different in very tricky ways. Social systems do not "act." Operation takes place in terms of them. Individuals, even classes of individuals, may act in roles explicitly or implicitly as representatives of social systems, but only in some of our more popular forms of metaphysics do states act as such. It is not enough to say that everyone knows this and that therefore it won't be confusing to use essentially confusing terminology in these respects. As a minimum one would have to state the differences between two such types of "actors" with great precision, and even then the fruitful use of jargon would argue for some use of differentiating terms. Failure to do so guarantees either anthropomorphizing social systems or question begging because of implicitly stated major points or both. Mr. Kaplan has carried this to the point of subtle parody in an appendix in which most of the major personality mechanisms, or whatever they are properly called, elucidated by psychoanalysis are mirrored in state "behavior." But do political systems have catharsis, cathexis, anti-cathexis, displacement, learning, recession, frustration, sublimation, projection, introjection, identification, and apathy and isolation—can the state love "her" father and be murderously jealous of her mother assuming "she" can find

them? It is the essence of Mr. Kaplan's parody that he is well aware of the fact that states as such do not act but that people act in the name of states. Even so the parody is not worth the cost. It is bound to mislead some who will take the author at face value and talk merrily on of cathecting states and the like ignoring the dual roles of the individuals who are in fact acting. Even for the sophisticated, however, there are problems. For them the parody is frustrating. They will know that the author really has something to say on this score, but by his metaphor he has not untangled the relation between the system of action and the actors in it—the special implications of their behavior in these contexts. By so doing he has caught our attention, tantalized us, and left us.

For reasons entirely unclear to me the author has also appended a treatment by reiteration of the subject of an objective scientific ethic. In the course of this he asserts [and who will disagree?] that there seems to be ample evidence that men on the whole value breathing, and by a mystery, which has always surpassed my understanding of both its content and general diffusion, gets from there to the assertion of a solution to the problem of a scientific ethic. Fortunately this appendix in no way affects the general argument or the general value of the rest of the volume.

I have been sharply critical of certain aspects of this work out of a very genuine sense of respect for what it attempts and what it begins. It is a pioneering attempt, and as such one should be tolerant of its shortcomings. One does no service to what the author most respects if he ignores the shortcomings, however. At this stage of development the whole effort is more endangered by sentimental, uncritical approbation than by outright rejection by those who feel that the leit-motif of the social analyst must be that we can either learn nothing or that we were taught it all two thousand years ago. Mr. Kaplan has earned vigorous criticism, respect for his ambitions, and thanks for having accepted the generally thankless burden of getting the matter started.

—MARION J. LEVY, JR.

A society which leaves no place for criticism of its own aims and methods by its component members has no chance to correct its errors and ailments, no chance to advance to new and better forms, and will eventually stagnate, if not die.

The Harvard Report on *General education in a free society*

ABSTRACTS OF CURRENT LITERATURE WITH INTERDISCIPLINARY IMPLICATIONS

Prepared by Members of the Staff of the Mental Health Research Institute

Landahl, H. D. **Theoretical considerations on potentiation in drug interaction.** *Bull. math. Biophysics*, 1958, 20, 1–23.

To account for some of the more important aspects of drug interaction we shall consider a model which can also account for certain general properties of the action of a single drug. A simple model in which there may be enzymatic detoxification of a drug is studied theoretically. The relation between time for appearance of an effect due to the drug and the size of the dose is found to contain the same parameters as the relation between the effectiveness of paired doses and the interval of time between doses. A similar situation holds when the drug is given at a constant rate.

When two drugs are administered together, their effect will depend on the manner of interaction, how much of each drug is given, which is given first, and on the interval of time between each administration. A number of plausible types of interaction is considered theoretically in terms of the model, analytical expressions being given for a number of cases. The interaction may be synergistic or antagonistic. In the former case the potentiation may be more than or less than additive depending on the order of delivery and on the time between injections. Methods for the estimation of the parameters from data are discussed. (Author's abstract)

Polanyi, M. **Problem solving.** *Brit. J. Phil. Sci.*, 1957, 8, 89–103.

The author provides a philosophical analysis of creative problem-solving behavior. Polanyi distinguishes at the outset between, on the one hand, purely random, systematic or reversible behavior, and on the other, behavior which cannot be charted or systematized in advance and proceeds toward a goal which, though marked out by the subject's conception of it, has yet never been seen in itself. Only the latter type of behavior is of interest to the author. Two operations which must always be tried jointly are postulated for the understanding of creative behavior. To quote the author: "We must (i) set out the problem in suitable symbols and continuously reorganise its representation with a view to eliciting some new suggestive aspects of it and concurrently (ii) ransack our memory for any similar problem of which the solution is known." Stated somewhat differently, the author seems to suggest that the problem must be set out (using suitable symbolisms) in some detail, thus allowing for the alternate selection of a great many features for consideration, and relating these features to known concepts and relations, slightly altering the latter in the process. The author then states: "The scope of these two operations will usually be limited by the student's technical facility for transforming the data in given ways and by the range of germane theorems with which he is acquainted." Stated in the language of cognitive theory, the author seems to suggest that the scope of creativity depends on the number of different cognitive subsets which the creator can bring to bear on a problem, which ultimately depends on the number of elements contained in his entire cognitive set and on the interrelationships existing between these. In summary, the problem-solving system outlined by Polanyi is one in which there is a "course," albeit a changing one, a control mechanism which has stored experiences sufficiently regular to provide a guide for the future, a set of responses also changing over time, and, finally, appropriate mismatch signals. Creativity by means of a random running through of all possible solutions is, on the part of the human subject at least, discounted by Polanyi. Finally, the

author points out that "the ideal of an impersonally detached truth must be abridged to allow for the inherently personal character of the act by which truth is declared." (*JG*)

Richter, M. N., Jr. Lehman, R. A. and Stillman, R. C. **Some structural aspects of concepts.** *J. Psychol.*, 1957, 44, 305–310.

The authors make an attempt at analyzing and comparing the logical systems involved in such varied conceptual phenomena as *abstract* thought, *stereotyped* thought, and *preconceptual* thought. Thus, one unified system is provided by which these otherwise seemingly quite disparate phenomena may be brought under one conceptual framework. *Abstract* thought is shown to depend on an explicit distinction between stipulative and empirical propositions. *Stereotyped* thought is characterized by an indiscriminate combination of the features of each of these propositions. *Preconceptual* thought is dependent on identity conceived in terms of a common predicate. Studies on the concept formation of children, on the thinking of schizophrenics, and on the conceptual processes involved in prejudice are cited and compared with the logical structure of the calculus of classes. (*JG*)

Stark, L. and Cornsweet, T. M. **Testing a servoanalytic hypothesis for pupil oscillations.** *Science*, 1958, 127, 588.

The response of the eye pupil to a sinusoidally varying light source has been measured by the authors. The movement of the pupil may be described in terms of standard servomechanism theory. The gain of the system is equal to the percentage of the change in light intensity which is compensated for by change in pupil diameter, and the phase shift is the time lag (in portions of a cycle) by which the pupil diameter follows the light intensity. The plot of phase shift versus gain as the frequency is changed yields a spiral diagram familiar to all servomechanism engineers as the Nyquist Diagram.

In any servomechanism, the system will oscillate at its natural frequency if the gain is increased to 1.0 or more. In the case of the eye pupil this is the frequency at which the phase lag is 180°. For the particular subject on which data is presented this corresponds to 1.2 cycles per second (or 72 cycles per minute). By focussing a small point of light on the margin of the pupil it is possible to induce epontaneous oscillations of the pupil (hippus). Measurements on several groups of normal subjects showed the mean frequency of spontaneous oscillations to be between 62 and 80 cycles per minute roughly within the predicted range.

Further experiments have been carried out by Stark and Baker in which the response of the pupil system has been altered by drugs. When the 180° phase cross-over frequency was changed from 1.5 to 0.9 cycles per second the high gain oscillation frequency shifted in a parallel fashion from 1.5 to 1.0 cycles per second.

The success of servoanalytic theory in explaining the mechanism of pupil oscillations suggests that similar explanations may be found for other neurological phenomena involving oscillations in the human system such as tremor, ataxia, clonus and nystagmus. (*WJH*)

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M. Richard Ferriman ("Screening Neuropsychiatric Patients by Means of the Kahn Test of Symbol Arrangement") received his bachelor's degree in education from the Ohio State University in 1948 and is now serving as a staff psychol, ogist at the United States Air Force Hospital, Wright-Patterson Air Force Base.

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Official Publication of the Mental Health Research Institute
The University of Michigan

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VOLUME 4 1959

Published quarterly by
The Mental Health Research Institute, The University of Michigan
at Mt. Royal & Guilford Aves., Baltimore 2, Maryland, and Ann Arbor, Michigan